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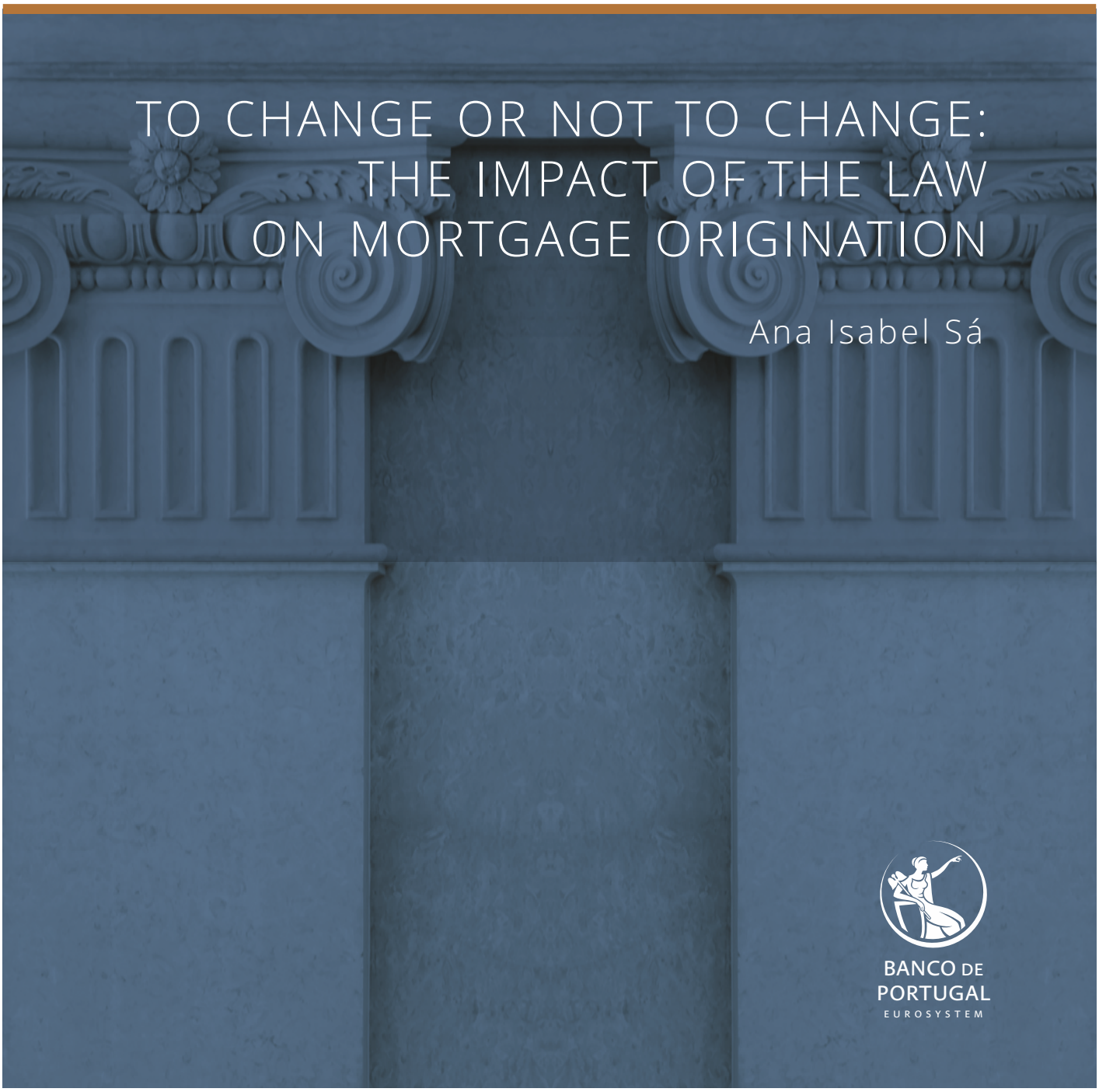

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Please address correspondence to
Banco de Portugal, Economics and Research Department
Av. Almirante Reis, 71, 1150-012 Lisboa, Portugal
Tel.: +351 213 130 000, email: estudos@bportugal.pt

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To change or not to change: the impact of the law on mortgage origination

Ana Isabel Sá
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Abstract
Differences in mortgage law have significant effects on loan characteristics at origination. Borrower-friendly laws impose higher costs and risks for lenders and, thus, induce effects on mortgage pricing and leverage. However, not all borrower-friendly laws have the same effects. This finding is established using loan-level data for the U.S. mortgage market between 2001 and 2011. Judicial foreclosure requirements imply higher mortgage interest rates due to higher recovery costs and activate the price channel. Recourse restrictions imply higher loan collateralization to compensate for the fewer recovery opportunities and activate the collateral channel.

JEL: E43, G21, G28, K25, K35

Keywords: mortgage, mortgage law, interest rate, mortgage pricing, judicial foreclosure, non-recourse, loan-to-value ratio, credit supply.

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E-mail: anaisabelsa@fep.up.pt

1. Introduction

Do changes in the law influence the mortgage market and loan characteristics? The intuitive answer might be yes, but there is little evidence of how mortgage law affects mortgage price and leverage. If the law imposes higher costs or risks for lenders, it is fair to assume that lenders may transfer the costs to the borrower or demand higher loan collateralization. Moreover, not all aspects of mortgage law induce the same effects. While some aspects may change the borrower's probability of default, others may only lead to higher recovery costs or longer recovery processes. When deciding to change or not to change the law it is, thus, important to have a reasonable knowledge of its different impacts. Many authors, at least since Pence (2006), have analyzed the influence of the law on mortgage credit, but few approach other outcomes besides loan amounts. This paper addresses this gap in the literature. By using loan-level data for the U.S. mortgage market, I trace how two aspects of the law—foreclosure procedure and deficiency judgments—influence the mortgage interest rate and the loan-to-value ratio at origination.

Similar to the extensive empirical literature (see, for example, Ghent and Kudlyak (2011), Mian *et al.* (2015), and Milonas (2017)), I start by taking advantage of mortgage law heterogeneity across states in the United States. I classify each state according to two aspects of the law: foreclosure procedure and deficiency judgments. On the one hand, the law might require a court's approval to initiate the foreclosure, which delays the process and makes it more costly. In these cases, I classify states as judicial. On the other hand, the law might not allow for deficiency judgments, which is usually known as a situation of non-recourse. It means that, if the house sale price is lower than the debt value, the lender cannot seize other assets or income of the borrower to recover the debt. Besides limiting the recovery opportunities, it might also affect the borrower's probability of default by encouraging strategic defaults. In these cases, I classify states as non-recourse.

One critical concern regarding mortgage heterogeneity is the verification of the desirable orthogonality properties along with state-level economic and social conditions. To address this issue, I check if states with distinct law classifications vary among other attributes that might influence the determination of the mortgage interest rate or the loan-to-value ratio at origination such as, for example, demographic factors. After inspecting the period from 2000 to 2016, I conclude that the orthogonality condition is not valid from 2012 onward. By combining this result with the loan-level data availability, I restrict the study to the period between 2001 and 2011.

I continue by developing a simple theory of mortgage origination to provide guidance on the impact of mortgage law on loan characteristics. The motivation of lenders and borrowers, while interacting in the mortgage market, is different. Lenders call for compensation that adequately prices the risk of the loan, whereas borrowers look for the loan that allows them to buy the house that best fits their preferences at the lowest cost. As a result, the market clearing process can be described by the joint determination of two variables. First, the mortgage interest

rate, as it reflects the price of the loan. Second, the loan-to-value ratio, as it measures the loan risk for lenders, and represents borrowers' simultaneous choice of house and loan amount. If the law implies differences in the cost and risk assessment of a mortgage, then the equilibrium solution may also show differences at the mortgage interest rate (price channel) or the loan-to-value ratio (collateral channel).

Under this framework, three questions arise: (i) If judicial states impose higher foreclosure costs, are their mortgage outcomes at origination different from the nonjudicial ones? (ii) If non-recourse states impose a smaller debt recovery and a bigger propensity to default, are their mortgage outcomes at origination different from the recourse ones? and (iii) Does the mortgage market adjust to differences in the law through mortgage interest rate (price channel) or loan-to-value ratio (collateral channel)?

To answer these questions, I study the differences in the mortgage interest rates and the loan-to-value ratio for more than 3 million observations on fixed-rate mortgages originated between 2001 and 2011. I find that the mortgage interest rate is 4.3 to 4.7 basis points higher in judicial states and, therefore, lenders transfer, at least, part of the extra costs to borrowers. I also find that the loan-to-value ratio is 0.42 to 0.58 percentage points lower in non-recourse states, meaning that lenders require more collateral to compensate for the lower recovery perspectives. The two aspects of the law lead to differences in loan characteristics but operate through different channels. While the foreclosure procedure activates the price channel, the deficiency judgment activates the collateral one.

But, if the effects of the non-recourse law only appear in a scenario of default, should we expect a homogeneous effect across loans with different expected loss levels? In particular, if lenders ask for higher collateralization in non-recourse states as a way to deter strategic default, should the impact be the same for collateral constrained and not collateral constrained borrowers? To expand the results obtained, I study the law impacts along the conditional distribution of the outcome variable for the collateral channel—the loan-to-value ratio. The results show that the impact of law for the different types of borrowers is not much different from the mean results, implying that the higher collateralization in non-recourse states is not due to the probability of strategic default. In other words, if lenders asked for more collateral to deter strategic defaults, then the impact would be stronger for collateral constrained borrowers, as they are closer to an event of negative equity. I complete the study by running some robustness checks and conclude that both impacts are fairly robust.

The extensive empirical literature explores the relationship between mortgage law heterogeneity in the United States and the credit supply but, to my knowledge, this is the first study to analyze price and collateral effects using a large mortgage loan-level dataset. Most authors measure the impact of the law on credit supply through loan size, and the result is not consensual. Pence (2006) applies a regression-discontinuity design at state boundary to loans originated between 1994 and 1995 and finds that loan sizes are 3 to 7 percent smaller in judicial states.

He only considers house values at county-level and does not include interest rate or collateralization data. Mian *et al.* (2015) report a similar methodology, but use loans originated in 2005 and data averaged by zip code. They find no evidence that average loan sizes or total lending are significantly lower in judicial states and argue that the effect described by Pence (2006) diluted over time. As for the impact of the law on credit supply through loan prices, most of the results arise from aggregate data at the state level. Ghent and Kudlyak (2011) and Pruszkowski (2017) find no difference in mortgage interest rates between recourse and non-recourse states and do not consider differences in the foreclosure procedure. Ambrose *et al.* (2004) use loan-level data from 1995 to 1997 provided by a national lender and with information on 26,179 loans. Although their central question is not the effect of law on prices and collateralization, the authors find puzzling results with lower interest rates and higher loan-to-value ratios in non-recourse states.

Another strand of literature emphasizes the role of mortgage law heterogeneity in the default and foreclosure rates. For the case of judicial foreclosure requirement, Mian *et al.* (2015) find that states that did not require judicial foreclosures were twice as likely to proceed with the foreclosure during the 2007 mortgage crisis. The effect was not driven by different default rates and does not persist after 2011. Desai *et al.* (2013) study the impact of the judicial requirement and non-recourse laws on default and foreclosures rates across different segments of the mortgage market. They conclude that the effects are stronger for subprime mortgages and adjustable rate mortgages. Gerardi *et al.* (2013) state that judicial requirements do not prevent foreclosures, and only delays the process. Melzer (2017) argues that the delay in foreclosing the house creates a debt-overhang problem with the deterioration of the property condition and consequent reduction in the collateral value. For the case of non-recourse law, many authors inspect whether or not negative equity is a sufficient condition to default in non-recourse states (strategic default) or if, on the contrary, liquidity constraints are the only source of default. Ghent and Kudlyak (2011) find that borrowers facing negative equity are more likely to default in non-recourse states, but only for home values above $\$200,000$. Guiso *et al.* (2013) find some evidence of strategic default and conclude that morality and fairness also plays a role in the decision. Demiroglu *et al.* (2014) inspect how judicial foreclosures and non-recourse interact in the likelihood of default on residential mortgages and conclude that this has a significant effect conditional on having negative equity.

Finally, mortgage law also relates to housing prices. Calomiris *et al.* (2013) find that judicial and nonjudicial states evidence different foreclosure start responses, conditional on equivalent house price and employment shocks. Mian *et al.* (2015) document the link between foreclosures and house prices, and use as an instrument the differences in the foreclosure rate in judicial and nonjudicial states. All in all, mortgage law heterogeneity generates a lot of research interest, but the conclusions regarding its impact at origination are not always obvious.

The paper is organized as follows. In Section 2, I describe the mortgage law heterogeneity in the United States. Section 3 presents a simple theoretical model of mortgage origination and the impact of the law. Section 4 presents the data and

empirical strategy. Section 5 provides results and Section 6 the robustness checks. Section 7 concludes.

2. Mortgage law heterogeneity in the U.S.

A mortgage contract gives the lender the right to foreclosure of the home in case of borrowers' default. In the U.S., the foreclosure procedure is governed mostly by state law and reveals a significant heterogeneity among states. After a depth review of U.S. mortgage law history, Ghent (2014) concluded that the roots of its heterogeneity go back to the nineteenth century and have no connection with economic factors. Despite several attempts to uniform the mortgage law at the national level, the cross-state variation existing today still influences both the borrower-lender relationship and the foreclosure outcome.[1]

Following the National Mortgage Servicer's Reference Directory (NMSRD) published by USFN (2018), I characterize the state-level differences in two aspects of the law: foreclosure procedure, and deficiency judgments.[2,3] First, as for the *foreclosure procedure*, when a borrower becomes delinquent on his mortgage and the lender wants to proceed with the foreclosure, the law may require a court's approval or allow a nonjudicial procedure. In states with judicial foreclosure, the average time and transaction costs to foreclose the property are generally higher. In states that allow procedures that do not require courts' approval (for example, power-of-sale foreclosures), lenders often choose a nonjudicial option to speed up the process and increase the recovery value. Thus, states that demand judicial procedures are more pro-borrower (Ghent 2014).

1. According to Ghent (2014), the bulk of laws developed through case law shows "remarkable persistence" along time, as they require changes to the civil code of procedures and cannot be promptly adjusted to the economic conditions. The few changes observed in the U.S. mortgage laws are frequently pro-borrower and relate to changes in statute law (such as changes in the redemption periods).

2. The NMSRD is a reference in the mortgage industry and provides information on state foreclosure processes and regulations. Although it does not systematically organize the changes that have occurred in U.S. mortgage law, it is possible to identify the most relevant changes over the last years for each state. For the desegregation of state-level differences, I consider the 50 U.S. states plus the federal District of Columbia (for simplicity, the District of Columbia is a *state*). Appendix A presents state classification for each mortgage law aspect. Appendix B presents differences in the classification across different sources and studies.

3. There are other state-level differences in law related to housing. For example, the homestead exemptions that shield a home from some creditors following the bankruptcy filling. The value of the exemption varies according to state and only protects households in case of unsecured debt. As it does not prevent a bank foreclosure, it does not directly apply to mortgages (secured loans). However, some authors recognize that homestead exemptions may influence mortgage default incentives if households simultaneously hold a positive equity mortgage and unsecured debt (see, for example, Li *et al.* (2011) and Hintermaier and Koeniger (2016)). As this indirect effect only applies to a sub-sample of households not identifiable in the data and depends on the household's future demand for unsecured credit, I decided not to consider differences in homestead exemptions as relevant at origination.

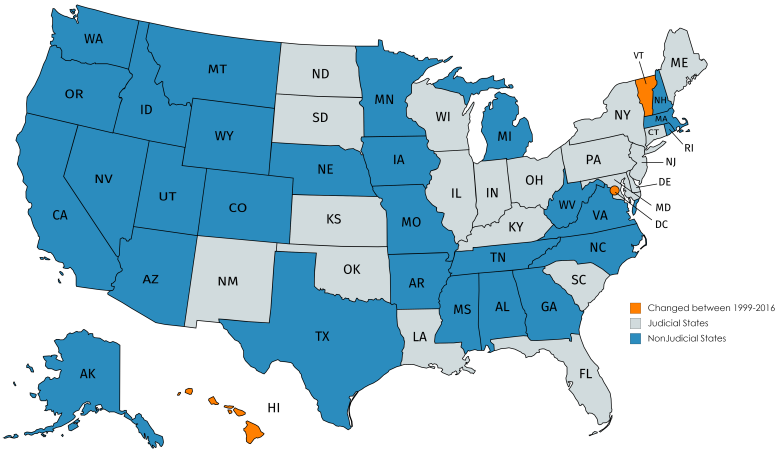


Figure 1: Distribution of judicial and nonjudicial states.

Note: This figure presents the distribution of judicial and nonjudicial states classified based on USFN (2018) data. States in lighter color require judicial foreclosures. For more details on the classification and changes occurred between 2000 and 2016, see Appendix A and B. Map created with *mapchart.net*.

The dummy variable $Judicial$ describes the foreclosure procedure classification. If a state requires a court's approval to proceed with foreclosure, then it is classified as judicial ($Judicial = 1$). All other cases are classified as nonjudicial ($Judicial = 0$).[4] Figure 1 shows a distribution map of judicial and nonjudicial states and emphasizes the changes registered between 2000 and 2016.[5] The distribution is balanced.

Second, as for the *deficiency judgments*, when the debt value exceeds the market value of the property, the lender may collect a deficiency judgment to pursue the borrower personally. With a deficiency judgment, the lender can seize the borrower's unsecured personal assets and future income to recover the debt value (Harris and Meir 2015). This possibility is usually known as recourse and might be automatic, require a judicial decision, or even be forbidden. States that do not allow recourse are more pro-borrower (Ghent 2014).

4. Including states that allow both judicial and nonjudicial foreclosures, but where nonjudicial are more common.

5. During this period, only three cases—District of Columbia, Hawaii, and Vermont—justified changes in the classification and all of them in the direction of judicial foreclosure.

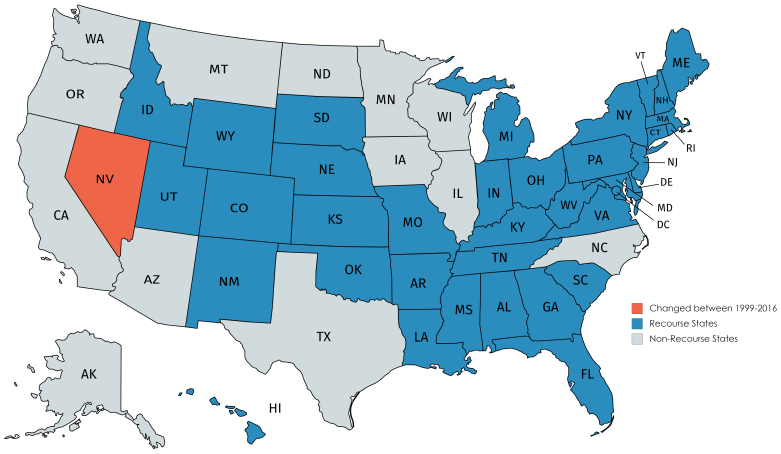


Figure 2: Distribution of recourse and non-recourse states.

Note: This figure presents the distribution of recourse and non-recourse states classified based on USFN (2018) data. States in lighter color do not allow recourse. For more details on the classification and changes occured between 2000 and 2016, see Appendix A and B. Map created with *mapchart.net*.

The dummy variable $NonRecourse$ describes the restrictions on deficiency judgments classification. If a state allows deficiency judgments through a process that has no major obstacles, then it is classified as recourse ($NonRecourse = 0$). All other cases are classified as non-recourse ($NonRecourse = 1$).[6] Figure 2 shows a distribution map of recourse and non-recourse states. The majority of states are recourse states.

2.1. Orthogonality of mortgage law differences to economic and social conditions

Relying exclusively on the distribution presented in Figure 1 and Figure 2 to explain the differences in the mortgage interest rate and the collateralization level might not be sufficient. One of the critical questions that arise is how distinct judicial and nonjudicial states are regarding other important dimensions. For example,

6. Exceptionally, the classification of the state of Nevada does not follow the NMSRD information. For the particular case of primary residence single-family mortgages, the state of Nevada passed a new law that abolished deficiency judgments for loans originated after October 1, 2009 (Li and Oswald 2017).

differences in mortgage pricing in judicial states may not be driven by law if households in judicial states also happen to have a lower income. The same might apply when comparing non-recourse to recourse states.

To ensure that the mortgage law differences are orthogonal to relevant economic and social conditions, one needs to show that states with different law classifications do not differ along with another attribute that independently influences the mortgage interest rate and the collateralization level.[7] I follow an approach similar to Mian *et al.* (2015) and test if judicial (non-recourse) states are significantly different in a set of economic and social attributes grouped into four categories: (1) demographic and income; (2) GDP; (3) housing market; and (4) mortgage market variables. By covering a substantial period from 2000 to 2016, I test the time consistency of the results.[8] Table C1 in Appendix C presents the results for the univariate regressions on whether a state requires judicial foreclosures. For most of the attributes from 2000-2011, judicial states do not significantly differ from the nonjudicial ones. The only exception is the fraction of population aged 65 or more years that is, on average, one percentage point higher in judicial states. As for 2012-2016 there are significant differences in the housing and mortgage market, which suggests that the assumption of orthogonality may not be valid during this period.

Table C2 in Appendix C presents the results for the univariate regressions on whether a state imposes non-recourse. For most of the attributes from 2000-2011, non-recourse states also do not significantly differ from the recourse ones. As exceptions are: the fraction of population with 65 or more years that is, on average, one percentage point lower in non-recourse states; the fraction of population that is black, which is, on average, six percentage points lower in non-recourse states; and the mortgage delinquency rate in 2004 and 2006, which is, on average, 30 basis points lower in non-recourse states. As for 2012-2016 there are also some signs of orthogonality assumption violation in the real GDP annual growth and in the housing and mortgage market.

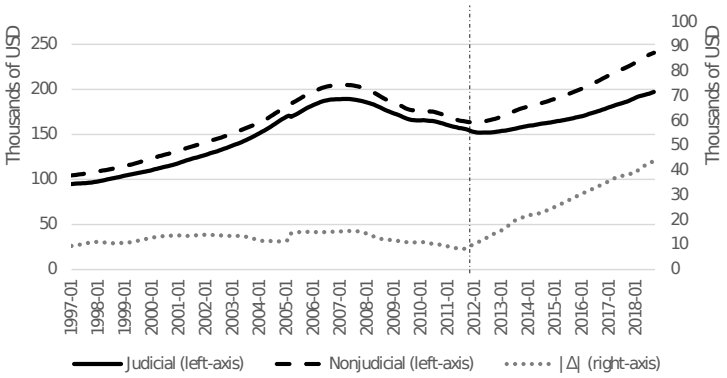
Taken together, these results suggest that differences in mortgage law are fairly independent of other attributes from 2000-2011. However, the same does not apply for 2012-2016.[9] To better understand this distinctive pattern, I inspect the evolution of the average price of a single-family residence by aspect of law. Figure 3 shows that prices in judicial states follow a parallel trend to those in nonjudicial states, at least up to 2011. From 2012 onward, there is a trend detachment that might independently influence the mortgage interest rate and the collateralization level. Figure 3 also shows similar results when comparing prices in non-recourse states

7. Several studies assume the orthogonality of the differences in mortgage law, at least for specific time periods (see, for example, Ghent (2014), Mian *et al.* (2015) and Milonas (2017)).

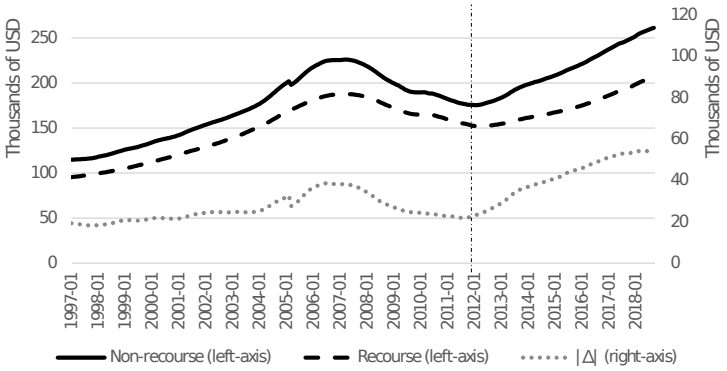
8. This approach excludes states where the law was changed between 2000 and 2016.

9. Appendix C presents the univariate regressions for the attributes evaluated every two years. To conclude that the break in the orthogonality occurred in 2011, I have run the univariate regressions for that year. However, for the sake of interpretability, I do not report yearly results in Appendix C.

to those in recourse states. Even if the break in the trend is not as evident as for the judicial foreclosure requirement, it is sufficient to question the validity of orthogonality. As a result, to securely assess the impact of mortgage law on the mortgage interest rate and the collateralization level, I restrict the analysis through 2011.



(a) A subfigure



(b) A subfigure

Figure 3: Average price for a single-family residence

Note: This figure shows the evolution of the simple-average price for a single-family residence according to the aspect of law based on Zillow data available on `https://www.zillow.com/research/data/`. The dotted line in the upper figure represents the difference between the average price in nonjudicial vs. judicial states. The difference is relatively stable up to 2012 and is consistent with the idea of parallel trends. After 2012, the trends detach. The dotted line in the lower figure represents the difference between the average price in non-recourse vs. recourse states. Although with a lower magnitude, the trends also detach after 2012.

3. A simple theory of mortgage origination

The basic idea behind the fundamentals of a market clearing process is that agents interact in the simultaneous determination of equilibrium price and quantities. When it comes to credit, and mortgage credit in particular, the dynamics are more complex. The obligations between borrower and lender do not end with the settlement of the mortgage contract. In fact, the loan origination is the beginning of a commitment that usually lasts for a long period, as it only ends with the debt repayment. To reduce the risk of the contract, lenders grant a secured loan, introducing a third variable in the simultaneous determination problem—the collateral value, which for mortgages is the house value. Instead of a standard price and quantities decision, there is a three-variable solution where the loan amount, the interest on the loan, and the house value are jointly determined (see, for example, Stiglitz and Weiss (1981) and Cerqueiro *et al.* (2016)). Recognizing these dynamics of mortgage credit, and to the extent of this simple theory of mortgage origination, I redefine the problem through a different perspective.

Let's start by considering the lender's maximization problem. Lenders seek to maximize profits for a certain risk level. The *price* of a loan is the mortgage interest rate. The *costs* of a loan are mainly the funding costs and capital charges.[10] The *risk* of a loan is its expected loss. All elements included in *price*, *costs*, and *risk* are in percentage values that do not depend on the loan amount. It is therefore reasonable to assume that lenders base their choices according to a risk-return per dollar lent, where the risk of a loan is not measured by the loan amount and house values individually, but rather by the ratio of both. From this perspective, the relevant decision variables for mortgage supply are the mortgage interest rate r^m, and loan-to-value ratio LTV.

And what about households? Can we assume that the decision variables are also the interest rate and the loan-to-value ratio? In a theoretical model of mortgage decision, Brueckner (1994) states that households simultaneously decide the loan amount and the house value. The loan-to-value ratio LTV is, thus, a relevant decision variable because households choose a house depending on how much the lender is willing to lend. The decision also depends on mortgage cost (proxied by the mortgage interest rate r^m), income level, savings, housing needs, and preferences.

For the purpose of this simple theory of mortgage origination, the demand and supply dynamics are redefined as a function of two variables—r^m and LTV. But, to understand the role of the law at mortgage origination, one need to take a step back and break up the concept of loan risk for the lender. The expected loss is the product of the probability of default (PD), and the loss given default (LGD).[11] The former reflects how likely is a borrower's default event. It is a function of the

10. The costs of a loan also include the operating costs. However, in a marginal cost perspective, operating costs tend to be irrelevant as they mainly include fixed overhead costs.

11. The exact definition of the expected loss includes the exposure at default (EAD). For mortgages at origination it represents the full loan amount (100%), and is therefore irrelevant.

borrower's credit score and the borrower's incentives to default, which might emerge along with the life of the contract. The latter represents the amount per dollar that the lender expects not to recover if the borrower defaults. It is a function of the loan-to-value ratio, the possibility to seize other assets, and the expected costs to be incurred in debt recovering (which might include operating, judicial, time, and other costs).[12] Hence, loan risk depends on loan and borrower's characteristics. All the other elements influencing the mortgage supply—such as capital charges, funding cost, or operating costs—do not depend on the loan characteristics, but rather on time and lender-specific factors.[13]

3.1. The impact of law

Mortgage law influences loan risk and cost, therefore, it generates direct effects on the mortgage demand and supply. From the interaction of both, one observes the total effects through the market equilibrium.

3.1.1. Direct effects — changes in the demand and supply. Stiglitz and Weiss (1981) argue that, in a loan contract, while firms focus on what happens under the no bankruptcy scenario, lenders pay attention to how the actions of the firm will influence its probability of default, and how can the lender recover the debt in case of default. Even though firms and households have different incentives, it is possible to establish some analogy as to the direct effects of mortgage law in the expected behavior of borrowers and lenders.

Consider a household that looks for funding to buy a house for primary residency. As the house satisfies a basic need, the borrower will have a strong commitment to pay the installments on time. Besides, failing the payment shatters the credit score, and makes it doubtful to get another mortgage. As a result, the default event is not a credible scenario at origination, and the borrower will not acknowledge the impact of the mortgage law in a potential foreclosure process.[14] But, even if the borrowers' decision process would acknowledge law criteria, it would only be binding if he was available to change his residency to another state.[15] It is, thus, reasonable to assume

12. As a reference, Repullo and Suarez (2004) derive a loan pricing equation where the equilibrium interest rate increases with the PD and the LGD. They also find that the equilibrium interest rates increase with the required level of capital and with the exposure to systematic risk, which, for the present study, are treated as time and lender-specific.

13. Capital charges represent the unexpected loan loss and, depending on the regulatory setting, might be loan-specific. For example, Basel III recommends that the capital charges on mortgages must consider the loan-to-value ratio (Basel Committee on Banking Supervision 2017). In the U.S. case, the Federal Reserve decided not to include this recommendation in the adoption of the Basel III criteria (Department of Treasury and Federal Reserve System 2013).

14. Exceptionally, one might assume that the non-recourse could influence the mortgage demand by relaxing the household concerns in case of default.

15. Exceptionally, law differences can be binding for households that live near the border of a state with different laws.

a limited influence of law heterogeneity on the mortgage demand. Contrarily, on the supply side, the effects are not negligible and dependent on the type of law.

Foreclosure procedure: In states that require judicial foreclosure procedures, lenders face additional costs and will, on average, take more time to conclude the foreclosure. Different authors recognize this increase in the foreclosure costs (such as Pence (2006), Mian *et al.* (2015), Shibut and Singer (2015), and others) which can go as high as 10% of the loan balance, according to Pence (2006). In a more general approach, Cerqueiro *et al.* (2016) also recognize that the legal mechanisms influence the value of the collateral as they determine when and how the secured assets can be seized. The higher costs in recovering the debt imply, *ceteris paribus*, a higher loss given default when compared to nonjudicial states. On the borrowers' side, judicial foreclosure procedures provide more protection and delay the foreclosure process, but it should not change borrowers' incentives to default. Overall, for loans with similar characteristics, I anticipate that the mechanism through which the expected loss increases in judicial states operates mainly through loss given default.

Restrictions on deficiency judgments: When recourse is not allowed, the value of the house given as collateral limits the debt recovery. The lender cannot seize other assets of the borrower, nor his income. With lower recovery opportunities, lenders face a higher loss given default, *ceteris paribus*, when compared to recourse states. Further, when recourse is not allowed, borrowers have more incentives to default when the loan balance exceeds the value of the house (negative equity). This event is well documented in the literature and is known as strategic default. Ghent and Kudlyak (2011) find that borrowers with properties appraised at more than $\$200,000$ are 30% more likely to default in non-recourse states. Overall, for loans with similar characteristics, I anticipate that the mechanism through which the expected loss increases in non-recourse states operates both through the probability of default and the loss given default.

With a higher expected loss in judicial and non-recourse states (borrower-friendly laws), one will expect that lenders adjust the pricing of mortgages by requiring, *ceteris paribus*, a higher risk premium. The more skin in the game, the more the lender calls for risk compensation. If mortgage law influences loan risk, then there should be a direct effect on the mortgage supply.

3.1.2. Total effects — changes in the mortgage market equilibrium. Direct effects determine how borrowers and lenders change their behavior in the presence of different law contexts, whereas total effects are the result of their interaction. Due to the joint determination of r^m and LTV, the adjustment mechanism might operate through two channels:

- Price channel, when the mortgage interest rate increases to reflect the higher risk and cost in borrower-friendly states (higher r^m);
- Collateral channel, when the collateral requirements increase to compensate for the increased risk in borrower-friendly states (lower LTV).

4. Data and empirical strategy

4.1. Data and descriptive statistics

This paper combines a variety of data sources. Besides the NMSRD for the mortgage law, I use the Freddie Mac's Single Family Loan-Level Dataset[16] for data on individual mortgages, and FRED Economic Data for data on market interest rates.

Freddie Mac Single Family Loan-Level Dataset comprises a portion of single family mortgages acquired or guaranteed by Freddie Mac. The data is publicly available for research purposes, which guarantees reproducibility and allows for study extension. It includes fully amortizing fixed-rate mortgages and has the comparative advantage of having information on interest rates.[17]

I restrict the sample period to loans originated between 2001 and 2011, to comply with the orthogonality assumption discussed in Section 2.1. The loan origination information is available by quarter/year. The selected sample includes mortgages granted for the purchase of a single-family primary residency, with a fixed interest rate, minimum amount of $\$50,000$ and original term between 25 and 35 years, and whose associated property locates in one of the states considered in Section 2.[18] In addition to the variables directly provided, I compute the borrower's monthly income ($Income$) through the monthly installments and the decomposition of the debt to income ratio. I also compute the loan-to-income ratio as the loan amount per dollar of monthly income (LTI).

All loans in the sample are fixed rate mortgages. To compare fixed interest rates from different periods and isolate the credit risk premium, I follow an approach similar to Basten *et al.* (2018) and compute the $Spread$ as the difference between the mortgage interest rate and the refinancing costs under full hedging of interest rate risk. In their work, the interest rate risk immunization strategy contemplates the repricing period of the mortgage as reference for the interest rate swap maturity. However, in the U.S., fixed rate mortgages have no interest rate resetting and no

16. Available in `http://www.freddiemac.com/research/datasets/sf_loanlevel_dataset.page` as of August 2018

17. It excludes government-insured mortgages, affordable mortgages, adjustable-rate mortgages, mortgages with credit enhancements other than primary mortgage insurance, and mortgages with not verified documentation.

18. Minor adjustments were made due to the presence of data inconsistencies. For further details please contact the author.

prepayment penalty, meaning that the fixed interest rate is valid up to the maturity date (25 to 35 years) or the loan prepayment, which ever one is first.[19] Given that the loan prepayment is unknown at origination, I assume that the quarterly average 30-year interest rate swap is the most adequate hedging instrument to compute the $Spread$.[20] Since mortgage rates are usually defined in advance of closing the deal, I lag the 30-year interest rate swap one quarter.

	mean	sd	min	max	Description
Spread	78	47	-304	496	Mortgage spread to previous period 30Y swap rate (bps)
Amount	180,557	92,792	50,000	1,403,000	Mortgage amount ($)
Value	238,181	144,902	50,000	5,214,286	House value ($)
LTV	79.38	13.75	6.00	105.00	Loan-to-value ratio (pp)
LTI	59.16	18.72	1.24	132.13	Loan-to-income ratio
CreditScore	730	54	300	850	Credit score
Income	3,391	2,693	413	298,465	Borrowers' monthly income ($)
DFirstTime	0.27	0.44	0	1	=1 if First-time homebuyer
DSingleBorrower	0.42	0.49	0	1	=1 if Single borrower
Judicial	0.46	0.50	0	1	=1 if in a judicial state
NonRecourse	0.37	0.48	0	1	=1 if in a non-recourse state
Observations		3,145,276			

Table 1. Summary Statistics

	Judicial		Nonjudicial		NonRecourse		Recourse	
	mean	sd	mean	sd	mean	sd	mean	sd
Spread	81	47	76	47	77	47	79	47
Amount	175,120	90,239	185,171	94,660	188,946	96,638	175,598	90,074
Value	230,047	139,253	245,084	149,181	251,139	154,399	230,521	138,413
LTV	79.65	13.71	79.15	13.77	78.99	13.80	79.61	13.71
LTI	58.69	18.64	59.57	18.77	60.11	18.66	58.60	18.73
CreditScore	729	54	731	54	731	54	730	55
Income	3,316	2,591	3,455	2,774	3,475	2,696	3,342	2,690
DFirstTime	0.27	0.44	0.27	0.44	0.28	0.45	0.27	0.44
DSingleBorrower	0.42	0.49	0.42	0.49	0.41	0.49	0.43	0.49
NonRecourse	0.19	0.39	0.53	0.50	0.23	0.42	0.59	0.49
Observations		1,443,938		1,701,338		1,168,552		1,976,724

Table 2. Summary Statistics by Judicial Foreclosure Requirement and by Non-Recourse Law

19. To immunize interest rate risk, lenders match the duration of assets and liabilities. In the determination of the mortgage duration, it is important to account for the possibility of principal prepayment. However, as it is an uncertain event that varies with the interest rate level and economic environment, the computation of the duration will depend on the specification of the option exercise (Mattey 2000).

20. As a robustness check, I assume that the prepayment occurs after 10 years and compute the $Spread$ as the difference between the mortgage interest rate and the 10-year Treasury yield. This approach is presented in Section 6.1.

Table 1 presents summary statistics of the loan-level data used.[21] The $3,145,276$ loans are distributed along 11 years and 44 quarters. The average loan has a $Spread$ of 78 basis points, an $Amount$ of $\$180,557$, a LTV of 79.38%, and a LTI of 59 times the monthly income. On average the borrower has a $CreditScore$ of 730 and an $Income$ of $\$3,391$. The sample is well distributed among judicial and nonjudicial states, with 46% of loans subject to judicial foreclosure. It is also fairly distributed among recourse and non-recourse states, with 37% of loans subject to non-recourse regime. Table 2 presents the summary statistics by aspect of law: judicial foreclosure requirement and non-recourse law.[22]

4.2. Data caveats and opportunities

The use of the Freddie Mac dataset is not without caveats. First, the dataset only includes loans acquired or secured by Freddie Mac, which might form a biased representation of the market. If the lender took advantage of having more information about the loans and, thus, sold the lower quality ones, than the dataset would entail a problem of adverse selection. Second, both government-sponsored enterprises (GSE) and purchasers of securitized mortgage-backed securities might not differentiate the risk of mortgage laws—intentionally or for lack of due diligence.[23] If the lender recognizes that the third parties are blind to the impact of law on risk, then he might also neglect this risk at origination if the intention is to sell the mortgage in the secondary market. As a cross reference of this second caveat, Mian *et al.* (2015) argue that the third parties' risk blindness along with the intensified presence of GSEs in the mortgage market, justify the weakening of the credit supply differences in nonjudicial states that were initially shown by Pence (2006).

These two caveats, however, do not jeopardize this study. At the origination, the lender may not have yet the intention to sell the mortgage to a third party.[24] At the sale, the risk is not necessarily fully transferred to Freddie Mac.[25]. And, as Pence (2006) argues, lenders and third parties interact in a repeated game, where a history of bad risk assessment might dictate the end of their relationship. It is,

21. Summary statistics by year presented in Appendix D.

22. As Table 1 shows, some loans have a negative $Spread$, which might look unusual. Besides being few loans, some negative $Spread$ are a result of computing it as the difference to the swap rate. I decided to maintain the loans in the sample, to avoid discretionary adjustments.

23. For example, the Freddie Mac's Single-Family Seller/Servicer Guide (Freddie Mac 2020) that establishes the requirements relating to the purchase of mortgages, makes no distinction of foreclosure procedure or deficiency judgments.

24. While some lenders proceed with the loan underwriting with the clear intention to sell the mortgage very soon after closing on the contract, others postpone the decision.

25. For example, loans with a LTV ratio in excess of 80% sold to Freddie Mac must comply with one of three options: (i) have a mortgage insurance provided by the lender or other entity on the portion that exceeds the 80% LTV ratio; (ii) keep all risks and costs of a borrower default on the lenders/seller-side; or (iii) be sold on a participation basis (Freddie Mac 2020)

therefore, not definite that loans on the dataset are different from the loans kept on the lenders' balance sheet.

But, most of all, even under the hypothesis that loans sold to Freddie Mac enclose loose credit standards, any evidence of differences derived by mortgage law heterogeneity found with this dataset imply that the effects on the entire market should be at least equal, but probably greater.

The use of the Freddie Mac dataset is also an opportunity. To my knowledge, former studies on the impact of law on mortgage outcomes disregard the impact on mortgage pricing (see, for example, Pence (2006) and Mian *et al.* (2015)), evaluate the price effects at aggregate level (see, for example, Ghent and Kudlyak (2011) and Pruszkowski (2017)), or use small private datasets (see, for example, Ambrose *et al.* (2004)). The lack of loan-level data with interest rate information makes it difficult to assess the price effects of mortgage law.[26] That is where the Freddie Mac dataset emerges as an opportunity. By providing information on interest rate, collateral value, borrower's risk, borrower's income, and others, the Freddie Mac dataset broadens the research opportunities.

As a final and general caveat, that applies to any dataset on U.S. mortgage data, is the strong presence of secondary market institutions and mortgage insurance companies, and the possible distortion that they might cause in the market outcomes, and in the loan characteristics. For example, Green and Wachter (2005) argue that these institutions blur the difference in the mortgage supply for high-risk and low-risk borrowers, while providing a solution for the Stiglitz and Weiss (1981) pooling problem. If this is the case, then any study on the impact of mortgage law heterogeneity in the U.S. most acknowledge that the mortgage risk assessment and pricing might be biased.

4.3. Empirical strategy

To identify the impact of mortgage law on loan characteristics, one would ideally want to observe the effects of an exogenous and not policy-oriented change in the law. In the absence of that event, I could alternatively study the interactions between borrowers and lenders at origination in different law contexts (from loan applications to bank responses) to disentangle the effects of law on mortgage demand and supply. Neither of both is possible. First, no event proxies a randomized experiment of a change in the law.[27] Second, the U.S. is the most representative

26. The most comprehensive source of publicly available information on loan origination in the United States is the Home Mortgage Disclosure Act (HMDA) data but, up to 2018, it does not include loan price information. The information on the interest rate, loan term, introductory rate period, and others were only available after the 2018 Home Mortgage Disclosure Act (effective in January 2018 for submissions due in March 2019) (FFIEC 2018).

27. As set in Section 2, Nevada passed legislation on October 1, 2009 that abolished recourse on mortgages granted with the purpose of purchasing a single-family home. However, this change in the law emerged as a response to the mortgage crisis and cannot be considered a random (not policy-oriented) change in law. Moreover, the data available for the transition period is not sufficient.

example of mortgage law heterogeneity, but no dataset includes simultaneously loan applications, loan pricing, and loan collateralization for a significant period.[28] Even for other geographic regions, detailed mortgage-level information that allows for a clear separation of mortgage demand and supply with price information is usually not publicly available.[29]

Given the availability of data, the solution includes a baseline model and a detailed approach that considers the conditional distribution of the LTV ratio.

4.3.1. Baseline model. The identification strategy departs from the observation of the mortgage law heterogeneity across U.S. states and the assumption of orthogonality to other economic and social factors from 2001 to 2011. If the mortgage law is randomly assigned, then there are no confounding factors in the treatment assignment, and the multivariate analysis is an appropriate method to evaluate the impact of the law. A second econometric challenge is the joint determination of the mortgage interest rate and the collateralization level, as discussed in Section 3. To deal with this endogeneity issue, I estimate the reduced-form equations for the mortgage interest rate spread and the loan-to-value ratio. This approach is similar to the one followed by Gambacorta and Mistrulli (2014). Equation (1) sets the estimation for $Spread$ and Equation (2) for the LTV

$$\begin{aligned} Spread_{i,j,t} = {} & \beta_{10} + \beta_{11} Log(Income)_i + \beta_{12} Log(Income)_i^2 + \beta_{13} LTI_i \\ & + \beta_{14} CreditScore_i + \beta_{15} DSingleBorrower_i + \beta_{16} DFirstTime_i \\ & + \beta_{17} Judicial_{i,t} + \beta_{18} NonRecourse_{i,t} \\ & + \Phi_{1t} + \Omega_{1j} + \varepsilon, \end{aligned} \tag{1}$$

$$\begin{aligned} LTV_{i,j,t} = {} & \beta_{20} + \beta_{21} Log(Income)_i + \beta_{22} Log(Income)_i^2 + \beta_{23} LTI_i \\ & + \beta_{24} CreditScore_i + \beta_{25} DSingleBorrower_i + \beta_{26} DFirstTime_i \\ & + \beta_{27} Judicial_{i,t} + \beta_{28} NonRecourse_{i,t} \\ & + \Phi_{2t} + \Omega_{2j} + \varepsilon, \end{aligned} \tag{2}$$

for loan i, seller j, and year t. The Φ are year fixed-effects to control for year nationwide shocks that might influence demand or supply. The Ω are seller fixed-effects to control for heterogeneity in interest rate setting and collateral requirements, due to the seller's specific characteristics of liquidity, capitalization, and relationship lending (Gambacorta 2008). As the error terms can display a potentially time-varying seller component from changes in seller's strategies, I cluster the residuals by seller.

28. See footnote 26.

29. Some exceptions include Basten *et al.* (2018) and Basten (2020) that use a sample of the Swiss mortgage market and Michelangeli and Sette (2016) that make use of simulations.

As discussed in Section 3, mortgage laws have a direct impact on the expected loss. Yet, the mechanism through which they influence the mortgage market equilibrium is distinct. The judicial foreclosure requirement implies higher costs in case of default (both time and monetary costs), but it is not expected to change the borrower's probability of default. As a result, in the market equilibrium, lenders might require a risk premium to compensate for the higher recovery costs ($\beta_{17} > 0$, price channel), or might input the extra recovery costs in the collateral value and require more collateral ($\beta_{27} < 0$, collateral channel). Both mechanisms are coherent with Cerqueiro *et al.* (2016) empirical evidence on collateral functions and the impact of legal tools.

The way through which non-recourse enforcement increases the expected loss is different. First, the probability of default might be higher due to the lack of borrower's incentives to comply with the contract in the presence of a scenario of negative equity caused by a downward housing market. Second, the loss given default might be higher, if lenders consider realistic expectations to seize the borrower's other assets and income in recourse states. As a result, in the market equilibrium, lenders might require a risk premium to compensate for the higher expected loss ($\beta_{18} > 0$, price channel) or require higher collateralization ($\beta_{28} < 0$, collateral channel), both to compensate for the higher loss given default or to decrease the probability of a negative equity scenario.[30] Table 3 summarizes the foreseen differences in the expected loss for the different mortgage laws, as well as the expected changes in the mortgage market equilibrium (total effects).

Mortgage Law	**Expected Loss**		**Mortgage Market Equilibrium**	
	Prob. of default (PD)	**Loss given default** (LGD)	**Price channel** (Spread)	**Collateral channel** (LTV)
Judicial	no effect	$+$	β_{17} 0 / $+$	β_{27} 0 / -
Non-recourse	no effect / $+$	$+$	β_{18} 0 / $+$	β_{28} 0 / -

Table 3. Expected effects of mortgage law

4.3.2. Law effects dependent on conditional distribution of the LTV ratio. The baseline approach focuses on the average effect of the law on mortgage pricing and collateralization level. It does not consider that the impact might diverge according to the conditional distribution of the LTV ratio. Put differently, the

30. The lower the LTV ratio, the less probable is a negative equity event.

baseline approach ignores that the impact of the law might be different for collateral constrained borrowers and not collateral constrained ones.

For example, it assumes that the impact of non-recourse law in the collateral requirements is the same for a not collateral constrained borrower that asks for a loan with a LTV ratio of 40%, and a collateral constrained borrower that asks for a loan with a LTV ratio of 85%. Yet, the impact of non-recourse law induced by the probability of strategic default is quite different. While for the not collateral constrained borrower the occurrence of a negative equity event requires a decrease in house value of more than 60%, for the collateral constrained borrower it only requires a decrease of more than 15%. The scenario of strategic default is, therefore, highly unlikely when the collateral constrain is not bidding. If lenders require more collateral to deter strategic default, then one would expect that the not collateral constrained borrower would not be affected.

To fix ideas, one should start by defining what is a collateral constrained borrower in the framework of the U.S. mortgage market. Usually, lenders prefer to grant conforming loans, as these loans are easily traded in the secondary market.[31] The Federal Housing Finance Agency (FHFA) sets, annually, the limits on the loan amount for conforming loans, and Freddie Mac and Fannie Mae set the guidelines on loan characteristics. Loans with LTV ratios above 80% can classify as conforming loans and be sold to Freddie Mac, but require additional protection in case of borrowers' default.[32] This criteria sets the mode for the typical loan, as going above 80% implies increasing costs. As a result, it is possible to classify each loan according to the conditional distribution of the LTV ratio and to the type of borrower:

- Type I (LTV ratio below 80%) - the borrower is not collateral constrained, as he could have asked for a higher loan to buy the house.
- Type II (LTV ratio of 80%) - the borrower might be collateral constrained, as he asked for the maximum loan amount that did not require additional protection to classify as conforming.
- Type III (LTV ratio above 80%) - the borrower is collateral constrained, as he chose to obtain a higher loan, even if that meant paying for private mortgage insurance.

Figure 4 presents the empirical cumulative distribution function of the LTV ratio for the loans included in the data. It shows pattern breaks around 30^{th} and 70^{th} quantiles, consistent with the above classification. Up to around the 30^{th} quantile, mortgages have an LTV ratio below 80% and show a concave distribution. From the 30^{th} quantile to a little before the 70^{th} quantile, mortgages have an LTV

31. Either through a government-sponsored enterprise or a private issue of mortgage-backed securities.

32. As set in Section 4.2.

ratio of 80%, and reflect a flat distribution. Afterwards, mortgages have an LTV ratio mostly between 80% and 95%, with higher values as exceptional cases.

If the effects of the law depend on the possible occurrence of a default event—which in turn depends on the type of borrower—is it reasonable to rely only on average effects estimated by Equation (2)? Or should we consider that the law effects vary according to the conditional distribution of the LTV ratio and the type of borrower? To test these hypothesis, I follow Koenker and Bassett (1978) and start by inspecting if the assumption of normality of residuals in Equation (2) is strictly satisfied.[33] If the distribution of the LTV ratio residuals does not approximate a normal distribution, I proceed by applying a quantile regression method. Besides not making assumptions on the parametric distribution of residuals, the quantile regression method allows for the estimation of explanatory variables' effects at different points of the LTV conditional distribution. It is also more robust in the presence of outliers as it considers median regression, rather than mean regression.

For the Type I borrowers, I analyze intratype effects at the 10^{th}, 15^{th}, 20^{th}, and 25^{th} quantiles, whereas for the Type II and III, I analyze the mid-points set by the 50^{th} and 85^{th} quantiles. I apply a quantile method with the same rationale as the baseline model.[34,35]

33. OLS methods model the conditional mean and assume a normal distribution of residuals, which ignores that responses can vary across the conditional distribution of the dependent variable. The violation of the normality assumption renders OLS estimators that are valid, but inefficient. In that case the use of an alternative approach is justified.

34. I use the STATA module *xtqreg* to estimate quantile regressions according to the method proposed by Machado and Santos Silva (2019) with seller and year fixed effects. Instead of clustering the errors by seller, I use bootstrap standard errors as recommended by Baum (2013).

35. I do not study the conditional distribution of the $Spread$ because there is no fundamental argument to do it. While the distribution of the LTV ratio relates to the collateral constrains of the borrower, the same does not apply to the price of mortgages.

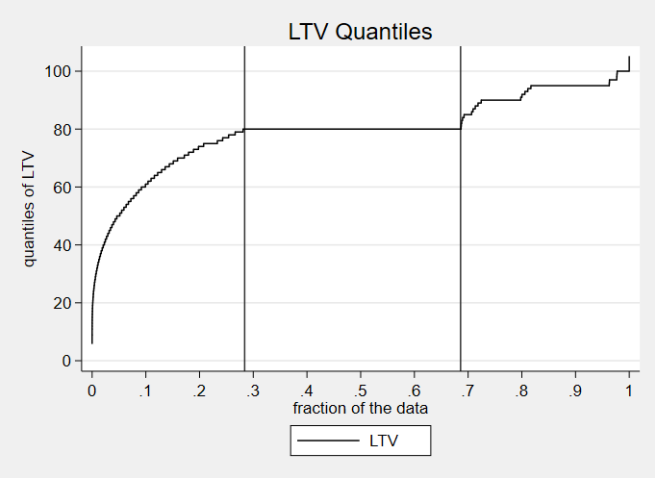


Figure 4: Cumulative Distribution of the LTV ratio.

Note: This figure presents the empirical cumulative distribution function of the LTV ratio. Approximately, 30% of the mortgages have a LTV ratio below 80% (Type I borrowers - not collateral constrained), 40% have a LTV ratio of 80% (Type II borrowers - might be collateral constrained), and 30% have a LTV ratio above 80% (Type III borrowers - collateral constrained).

5. Results

5.1. Baseline model results

Table 4 presents the OLS estimation of the reduced-form Equations (1) and (2), with and without seller fixed-effects. As for the price channel, Equation (1), judicial requirements significantly increase the mortgage spread (β_{17} greater than 0), whereas non-recourse law does not affect the price (β_{18} not significantly different from 0). Lenders require monetary compensation for the increased foreclosure costs—induced by the judicial requirements—but not for the lower recovery opportunities and increased probability of default—originated by the non-recourse law. On average, the mortgage interest rate in judicial states is approximately 4.3 to 4.7 basis points higher than in nonjudicial ones.

As for the collateral channel, Equation (2), judicial requirements do not decrease the LTV (β_{27} not significantly different from 0), whereas non-recourse law implies a lower LTV ratio (β_{28} lower than 0). Lenders require more collateralization given the lower recovery opportunities and increased probability of default—originated by the non-recourse law—but not for the increased foreclosure costs—induced by the judicial requirements. On average, the loan-to-value ratio is approximately 0.42 to 0.58 percentage points lower in non-recourse states.

But why is the collateral channel silent in judicial states, and why is the price channel functioning? According to the evidence gathered by Cerqueiro *et al.* (2016),

	Spread	**Spread**	**LTV**	**LTV**
LogIncome	-28.709**	-27.960**	49.094***	49.327***
	(9.617)	(9.058)	(2.717)	(2.709)
LogIncome^2	1.131*	1.108*	-3.066***	-3.063***
	(0.540)	(0.502)	(0.165)	(0.166)
LTI	-0.286***	-0.281***	0.041***	0.046***
	(0.015)	(0.014)	(0.006)	(0.006)
CreditScore	-0.133***	-0.132***	-0.055***	-0.055***
	(0.009)	(0.009)	(0.002)	(0.002)
DSingleBorrower	3.646***	3.566***	1.995***	2.015***
	(0.493)	(0.474)	(0.081)	(0.075)
DFirstTime	1.746*	2.512*	3.700***	4.066***
	(0.693)	(0.917)	(0.225)	(0.162)
Judicial	4.698***	4.305***	0.303	0.167
	(0.249)	(0.207)	(0.186)	(0.170)
NonRecourse	-0.050	0.219	-0.583***	-0.421**
	(0.170)	(0.234)	(0.129)	(0.138)
FE Year	yes	yes	yes	yes
FE Seller	no	yes	no	yes
N	3,145,276	3,145,276	3,145,276	3,145,276
R^2	0.327	0.332	0.096	0.106

Table 4. Effects of law on Spread and LTV

This tables presents coefficients of the OLS specification for $Spread$ and LTV on law dummies—$Judicial$ and $NonRecourse$. Standard errors are clustered by seller. Coefficients marked with ***, **, * and $^+$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.

both should be active. But, if only the price one is effective, one can conclude that lenders consider the extra cost from judicial foreclosures as an operating limitation that makes the loan activity more expensive, rather than a devaluation of the collateral value per se. Lenders do not input the judicial cost to the collateral value.

And why is the price channel silent in non-recourse states, and why is the collateral channel functioning? One possible reason for a silent price channel is that lenders in recourse states usually face judicial costs to seize borrower's other assets and income. These costs can offset the value of the recovery opportunities and apparently silence the price channel. As for an effective collateral channel, there are two possible reasons. First, non-recourse laws imply a zero recovery beyond the house value, therefore lenders require a lower LTV ratio to, *ceteris paribus*, increase the protection against default. Second, lenders might want to avoid strategic defaults by reducing the probability of an event of negative equity.

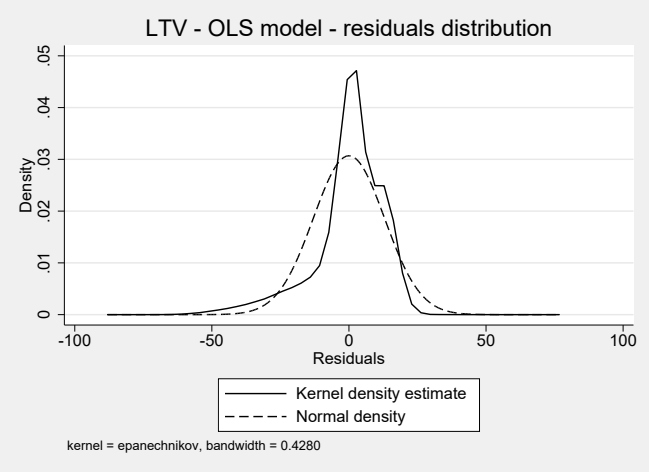


Figure 5: Residuals distribution on the LTV ratio regression

Note: This figure presents the distribution of the residuals on the estimation of Equation (2) with year and seller fixed effects. The estimated residuals (Kernel density estimate) violate the assumption of normality distribution represented by the dash line.

With higher collateralization, it is less probable that the debt value will exceed the house value during the life of the contract.

The latter argument provides one of the motivations for the next subsection. If lenders require more collateral in non-recourse states as a way to deter strategic default, then the effect of law should differ for different points of the LTV ratio conditional distribution.

5.2. Law effects dependent on the conditional distribution of the LTV ratio

Figure 5 presents the distribution of the residuals of Equation (2) with year and seller fixed-effects. The distribution of the residuals on the LTV ratio regression violates the normality assumption.[36] As a result, OLS estimators in Table 4 are valid but inefficient, which justifies the use of a quantile regression method.[37]

Table 5 presents the estimation results for the LTV ratio quantile regression with seller and year fixed effects. As expected, the effects of explanatory variables vary across quantiles, and the differences are particularly relevant for the variable

36. Normality tests, like Shapiro-Wilk W or Shapiro-Francia, are not valid for large samples. The normality of residuals distribution can only be validated through visual inspection.

37. The conditional distribution of $Spread$ is not relevant for Section 4.3.2 argument. Nevertheless, E presents the distribution of the residuals of Equation (1) with year and seller fixed-effects. There is no evidence of normality assumption violation for the $Spread$ regression.

$Income$. On the other hand, the effect of non-recourse law shows little divergence across quantiles, and not in the expected direction. The collateral requirements due to non-recourse law are higher for not collateral constrained borrowers than for collateral constrained ones. This result overrules the hypothesis that lenders require a lower LTV ratio in non-recourse states to deter borrowers from strategic default. If it was true, the effects should be stronger for collateral constrained borrowers (Type III) because they are the first to strategic default in a scenario of a falling housing market. For Type III borrowers, the LTV ratio is 0.30 percentage points lower in non-recourse states, whereas for Type II borrowers, it is 0.41 percentage points lower. The results are even more striking for differences within the Type I borrowers. If lenders demand more collateral to deter strategic default, then the effect of non-recourse on not collateral constrained borrowers with loans with low LTV ratios should be almost negligible. Yet, the impact of non-recourse within the Type I borrowers is decreasing with the quantiles. For borrowers belonging to the 10^{th} quantile, the LTV ratio is 0.58 percentage points lower in non-recourse states, whereas for borrowers belonging to the 20^{th} quantile, it is 0.49 percentage points lower.

	OLS	Type I Q(0.10)	Type I Q(0.15)	Type I Q(0.20)	Type I/II Q(0.25)	Type II Q(0.50)	Type III Q(0.85)
LogIncome	49.327***	119.644***	95.423***	79.919***	69.671***	42.610***	-2.516
	(2.709)	(4.688)	(3.459)	(2.252)	(2.421)	(1.630)	(1.599)
LogIncome2	-3.063***	-7.163***	-5.750***	-4.847***	-4.249***	-2.671***	-0.040
	(0.166)	(0.291)	(0.216)	(0.137)	(0.150)	(0.099)	(0.098)
LTI	0.046***	0.148***	0.113***	0.090***	0.075***	0.036***	-0.029***
	(0.006)	(0.008)	(0.007)	(0.008)	(0.005)	(0.005)	(0.005)
CreditScore	-0.055***	-0.069***	-0.064***	-0.061***	-0.059***	-0.054***	-0.045***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
DSingleBorrower	2.015***	3.538***	3.013***	2.677***	2.455***	1.869***	0.892***
	(0.075)	(0.160)	(0.129)	(0.085)	(0.093)	(0.069)	(0.058)
DFirstTime	4.066***	7.520***	6.330***	5.569***	5.065***	3.736***	1.519***
	(0.162)	(0.375)	(0.232)	(0.216)	(0.165)	(0.138)	(0.207)
Judicial	0.167	0.221	0.203	0.190	0.183	0.161	0.126
	(0.170)	(0.234)	(0.218)	(0.169)	(0.186)	(0.137)	(0.153)
NonRecourse	-0.421**	-0.582**	-0.526**	-0.491**	-0.467***	-0.405***	-0.302***
	(0.138)	(0.200)	(0.175)	(0.190)	(0.136)	(0.120)	(0.073)
FE Year and Seller	yes	yes	yes	yes	yes	yes	yes
N	3,145,276	3,145,276	3,145,276	3,145,276	3,145,276	3,145,276	3,145,276

Table 5. Effects of law on the conditional distribution of LTV

This tables presents coefficients of the OLS and the Quantile Regression specification for LTV on law dummies—$Judicial$ and $NonRecourse$. Quantiles defined according to the type of borrower. Standard errors are bootstrapped by seller with 100 repetitions. Coefficients marked with ***, **, * and $^{+}$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.

If the argument of strategic default avoidance is not valid to justify the higher collateralization level in non-recourse states, then it only remains the justification that lenders might require less collateral in recourse states because they have realistic expectations of seizing the borrower's other assets and income.

As for the impact of judicial foreclosures, there are no differences across quantiles and the effect is null. This means that the collateral channel is not activated to compensate for judicial foreclosure requirement, independently of the type of borrower.

6. Robustness tests

As robustness checks, I run three additional models. First, I use an alternative definition of $Spread$ that considers the hypothesis of prepayment of principal. Second, I test if the effects previewed in the baseline model prevail when we consider the secondary mortgages associated with the same property. Third, I test the time dependency of the results obtained.

6.1. Alternative definition of Spread

The definition of $Spread$ used in this study assumes that by deducting the refinancing costs under full hedging of interest rate to the mortgage interest rate, one obtains the credit risk premium. By using the 30-year interest rate swap as reference rate, it does not consider the possibility of mortgage prepayment in the hedging strategy. Yet, in the U.S mortgage market prepayment is frequent and motivated by a variety of reasons.[38]

To address this concern, I redefine $Spread$ as the difference between the mortgage interest rate and the lagged quarterly average 10-year Treasury yield and name it $Spread2$. I use the 10-year Treasury yield for three reasons: first, several studies use Treasury rates as a reference for computing credit risk premium (for example, Krainer and LeRoy (2010)); second, lenders use Treasury securities as benchmarks to set up mortgage rates (Justiniano *et al.* 2016); and third, borrowers usually repay the mortgage before the maturity and ten years is a good proxy for principal prepayment (Ambrose *et al.* 2004).

This approach, however, does not control for the co-movement between mortgage and Treasury rates, neither controls for the term structure. To account for the pattern of the expectations for interest rates, I follow an approach similar

38. Chernov *et al.* (2018) study the mortgage prepayment risk in the US for the period between 1998 and 2014. They find that the average implied prepayment rate is 25.13% and the average empirical prepayment rate is 20.96%. Hall and Maingi (2019) point out some of the reasons that lead to mortgage termination, which include: refinancing with a new mortgage with better conditions; refinancing with a new mortgage with a higher balance; paying off the mortgage as the better strategy when comparing alternative savings investments; paying off the mortgage to exchange the house; defaulting, and homeowner's death.

to Justiniano *et al.* (2016) and control for four additional variables—$Level$, $Slope$, $Curvature$, and $Volatility60d$. The first three represent the quarterly average of the term-structure factors for Treasuries ($Level$, $Slope$, and $Curvature$), and are obtained through principal components analysis. The latter represents the quarterly average of a volatility indicator ($Volatility60d$) and is computed as the realized volatility of the daily 2-year Treasury yield over a rolling 60-day window.

Table F1 in Appendix F presents the results of the estimation of the baseline model for the $Spread2$. The results are qualitatively similar to the ones presented on Table 4, which confirms Section 5.1 conclusions.

6.2. Combined LTV ratio

At the origination of a purchase mortgage loan, borrowers occasionally ask for a secondary mortgage that is associated with the same property as the primary mortgage, but that can have different characteristics, such as interest rate and maturity. In this case, it is reasonable to assume that lenders evaluate the risk by considering the entire exposure associated with the mortgaged property. To test if the impact of law remains the same when we consider the entire exposure, I re-estimate Equation (2) by using the combined loan-to-value ratio ($CLTV$) at origination instead of the LTV ratio.

Table F2 in Appendix F presents the OLS results for the $CLTV$ regression.The results are consistent with the ones presented in Table 4. Judicial foreclosure requirements do not impact the collateralization level, whereas the non-recourse law leads to a lower $CLTV$ ratio of approximately 0.42 to 0.50 percentage points. These results confirm the conclusions presented in Section 5.1.

6.3. Law effects dependent on the time frame

The baseline approach includes year fixed effects that control for nationwide shocks, and captures time-specific dynamics of the mortgage market. Nevertheless, to inspect Mian *et al.* (2015) argument that the credit supply effects related to judicial foreclosures requirement found by Pence (2006) have weakened over time[39], I re-estimated the reduced-form equations by interacting law with year dummies.

Table F3 in Appendix F presents the OLS results for the $Spread$ and LTV regressions with year-law interactions. As for the price channel, judicial requirements significantly increase the mortgage spread for most of the years, and the effect varies from 1.5 to 6.5 basis points. The only exceptions are 2010 and 2011, for which the effect is not significantly different from zero. As for the collateral channel, the impact of non-recourse on the LTV ratio is also significant for most of the sample, and varies from 0.33 to 0.78 percentage points. The only exceptions are 2001, 2009 and 2010.

39. Mian *et al.* (2015)'s argument focus on the period between 2000 and 2005.

Despite the evidence of a price and collateral channel for most of the sample period, one cannot disregard the hypothesis that, in some periods, lenders might assign a low probability to scenarios of substantial house prices declines (Gerardi *et al.* 2013) and, thus, not penalize judicial foreclosures and non-recourse. By inspecting the evolution of Case-Shiller U.S. National Home Price Index, there is anecdotal evidence that points to the coincidence of a depressed housing market with the verification of silent price and collateral channels.[40]

7. Conclusion

Mortgage laws influence the debt recovery process in case of default and, thus, impact the cost and risk of mortgages for lenders. Depending on the type of law, the effects might materialize in a higher probability of default, a higher loss given default, or both. By focusing on the U.S. mortgage market, I analyze the judicial foreclosure requirement and the non-recourse enforcement, as borrower-friendly laws that increase the mortgage cost and risk. I use a loan-level dataset to assess how the law influences loan characteristics at origination, and consider two possible channels: price and collateral channels.

I find that judicial foreclosures activate a price channel, with the mortgage interest rate being 4.3 to 4.7 basis points higher in judicial states. The results suggest that lenders charge a premium to compensate for higher monetary and time costs incurred in judicial processes. This effect is fairly robust, even if it shows some variation over time. On the other hand, there is no evidence that judicial foreclosures lead to higher collateral requirements.

As for the non-recourse law, I find that it activates a collateral channel, with mortgages in non-recourse states having a lower loan-to-value ratio of about 0.42 to 0.58 percentage points. I also find that the collateral requirements in non-recourse states are not higher for collateral constrained borrowers, which means that they do not arise as a way to deter strategic default. Higher collateralization levels in non-recourse states derive from lower opportunities to recover the debt as the lender cannot seize other borrower's assets and income, other than the house given as collateral.

These results provide new insights into the consequences of changing mortgage law. This contribution is key for any policy agent that has the power to suggest or enforce changes to the mortgage law, whether to protect households or to strengthen the mortgage market. However, it is not in this study objectives to set any normative conclusion on what type of law should be considered as more beneficial. Whether it is better to have a judicial or nonjudicial foreclosure procedure ultimately depends on the objectives of the policymaker.

40. Figure F1 in Appendix F shows the evolution of Case-Shiller U.S. National Home Price Index and emphasizes the years 2009, 2010 and 2011.

The policymaker might want to protect borrowers by guaranteeing an impartial assessment of the foreclosure or by extending the time of a primary residence foreclosure process. In that case, the policymaker prefers a judicial foreclosure procedure, but what he needs to acknowledge is that these household's benefits come at the cost of a higher price for the mortgage. Alternatively, the policymaker might want to protect borrowers that are in a situation of fragility after a house foreclosure by limiting the access of lenders to borrower's income and other assets. In that case, the policymaker prefers a non-recourse law, but what he needs to acknowledge is that lenders will protect themselves by requiring higher collateralization levels, which will reduce households' access to the mortgage market.

In the end, this study appeals to the idea that *there ain't no such thing as a free lunch*. So, if policy makers are considering changing the mortgage law, they must be aware of its effects on the price and collateralization level of mortgages.

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Appendix A: Classification of states by mortgage law aspect

This appendix presents the current classification of states by mortgage law aspect, as well as the changes from 2000 to 2016. The column $Judicial$ describes the foreclosure procedure classification, where $Judicial = 1$ represents mandatory judicial foreclosures. The column $NonRecourse$ describes the restrictions on deficiency judgments, where $NonRecourse = 1$ means that deficiency judgments are not allowed.

State		Time	Judicial	NonRecourse
ALABAMA	AL	all	0	0
ALASKA	AK	all	0	1
ARIZONA	AZ	all	0	1
ARKANSAS	AR	all	0	0
CALIFORNIA	CA	all	0	1
COLORADO	CO	all	0	0
CONNECTICUT	CT	all	1	0
DELAWARE	DE	all	1	0
DIST. OF COLUMBIA	DC	up to 2010	0	0
DIST. OF COLUMBIA	DC	2011 onward	1	0
FLORIDA	FL	all	1	0
GEORGIA	GA	all	0	0
HAWAII	HI	up to 2010	0	0
HAWAII	HI	2011 onward	1	0
IDAHO	ID	all	0	0
ILLINOIS	IL	all	1	1
INDIANA	IN	all	1	0
IOWA	IA	all	0	1
KANSAS	KS	all	1	0
KENTUCKY	KY	all	1	0
LOUISIANA	LA	all	1	0
MAINE	ME	all	1	0
MARYLAND	MD	all	1	0
MASSACHUSETTS	MA	all	0	0
MICHIGAN	MI	all	0	0
MINNESOTA	MN	all	0	1
MISSISSIPPI	MS	all	0	0
MISSOURI	MO	all	0	0
MONTANA	MT	all	0	1
NEBRASKA	NE	all	0	0
NEVADA	NV	up to 2009	0	0
NEVADA	NV	2010 onward	0	1
NEW HAMPSHIRE	NH	all	0	0
NEW JERSEY	NJ	all	1	0
NEW MEXICO	NM	all	1	0
NEW YORK	NY	all	1	0
NORTH CAROLINA	NC	all	0	1
NORTH DAKOTA	ND	all	1	1

Table A1. Mortgage law by state

State		Time	Judicial	NonRecourse
OHIO	OH	all	1	0
OKLAHOMA	OK	all	1	0
OREGON	OR	all	0	1
PENNSYLVANIA	PA	all	1	0
RHODE ISLAND	RI	all	0	0
SOUTH CAROLINA	SC	all	1	0
SOUTH DAKOTA	SD	all	1	0
TENNESSEE	TN	all	0	0
TEXAS	TX	all	0	1
UTAH	UT	all	0	0
VERMONT	VT	up to 2006	0	0
VERMONT	VT	2007 onward	1	0
VIRGINIA	VA	all	0	0
WASHINGTON	WA	all	0	1
WEST VIRGINIA	WV	all	0	0
WISCONSIN	WI	all	1	1
WYOMING	WY	all	0	0

Table A1. Mortgage law by state (cont.)

Appendix B: Comparison of classification of states by mortgage law aspect

This appendix presents the comparison of classification of states by mortgage law aspect across different sources and studies, namely Mian *et al.* (2015), Ghent and Kudlyak (2011), and RealtyTrac.com[41]. Table B1 shows the differences in the foreclosure procedure classification. Table B2 shows the differences in the non-recourse classification.

State		This study	Mian et al (2015)	Ghent and Kudlyak (2011)	Realty Trac.com	Justification
DIST. OF COLUMBIA	DC	0/1	0	0	0	The change in the judicial foreclosure requirement in the District of Columbia started with the "Saving D.C. Homes from Foreclosures Emergency Amendment Act of 2010" effective on November,17 2010. During the discussion period the majority of residential foreclosure activity become judicial due to the risks involved. After some changes, the Act became law on November 4, 2013.
HAWAI	HI	0/1	0	0	0	With the "Act48" SLH 2011 of May, 5 2011, lenders where discouraged to pursue nonjudicial foreclosures. Most foreclosures become judicial.
MASSACHUSETTS	MA	0	1	0	1	Foreclosures in Massachusetts require a two-stage procedure. First, a judgment from the land court declaring that the mortgagors are not under protection of the Service members Civil Relief Act (SCRA). Second, a nonjudicial or judicial foreclosure depending on the existence of a power of sale clause. I consider the first stage as not significant for the classification of judicial and nonjudicial foreclosure, meaning that, following the criteria on the other states I classify Massachusetts as a state that does not require judicial foreclosure.

Table B1. Differences in the judicial foreclosure classification

41. Data available on `https://www.realtytrac.com/real-estate-guides/foreclosure-laws/` as of January, 28 2020.

State		This study	Mian et al (2015)	Ghent and Kudlyak (2011)	Realty Trac.com	Justification
NEBRASKA	NE	0	1	0	1	Nebraska allows for nonjudicial foreclosures for a deed of trust, but requires judicial foreclosure for mortgages. As in this paper I use a broad definition of mortgage that includes the deeds of trust, and following the criteria on the other states, I classify Nebraska as a state that does not require judicial foreclosure.
OKLAHOMA	OK	1	0	0	1	Oklahoma allows for nonjudicial foreclosures but are extremely rare, due to certain restrictions. As most foreclosures are judicial, I classify Oklahoma as a state that requires judicial foreclosure.
SOUTH DAKOTA	SD	1	0	0	1	South Dakota allows for nonjudicial foreclosures but are frequently not used, due to uncertainty in the foreclosure process. As most foreclosures are judicial, I classify South Dakota as a state that requires judicial foreclosure.
VERMONT	VT	0/1	1	1	1	After 2006, nonjudicial foreclosures were discouraged in Vermont due to a revision of the foreclosure statute. The 2006 foreclosure stature establishes that nonjudicial foreclosures are only possible if there is no equity in the subject property, which is burdensome to prove. I classify Vermont as a state that does not require judicial foreclosure up to 2006 and as a state that requires judicial foreclosure from 2007 onward.
WISCONSIN	WI	1	0	1	1	Although Wisconsin allows for nonjudicial foreclosures, most foreclosures are judicial.

Table B1. Differences in the judicial foreclosure classification (cont.)

State		This study	Ghent and Kudlyak (2011)	Justification
ILLINOIS	IL	1	0	Ghent and Kudlyak (2011) recognize that deficiency judgments are rarely granted, but classify the state as recourse by arguing that the possibility of personal recourse may be enough to deter strategic default. I do not follow their argument because the judicial approval of the property sale frequently implies that the lender waves her right to a deficiency judgment. If the sale is conditional on waving the deficiency judgments, then the lender is not protected by personal recourse.
TEXAS	TX	1	0	Ghent and Kudlyak (2011) recognize that the state law allows deficiency judgments, even if the large homestead exemption may be an obstacle. I do not follow their argument because besides the almost unlimited homestead exemption, deficiency judgments are frequently noncollectable due to the fact that the borrower is "judgment proof".

Table B2. Differences in the non-recourse classification

Appendix C: Orthogonality of mortgage law differences to social and economic conditions

This appendix presents the results of the univariate regressions of a set of state attributes on the mortgage law aspects. Table C1 shows if judicial states also significantly differ in other economic and social attributes. Table C2 shows if non-recourse states also significantly differ in other economic and social attributes.

	Judicial	**N**	R^2
Demographics and Income Variables			
Fraction with 65 years or more, 2008-14	0.0106 *	47	0.108
	(0.0043)		
Fraction with less than high school, 2008-14	-0.0058	47	0.011
	(0.0077)		
Black fraction, 2008-14	0.0270	47	0.020
	(0.0282)		
Hispanic fraction, 2008-14	-0.0054	47	0.001
	(0.2979)		
Poverty fraction, 2008-14	-0.0005	47	0.000
	(0.0090)		
Fraction with personal income less than $25k, 2008-14	-0.0060	47	0.007
	(0.1070)		
Fraction with household income less than $25k, 2008-14	0.0039	47	0.002
	(0.0120)		
Fraction with income less than $25k, 2008-14	0.0029	47	0.001
	(0.0119)		
Unemployment rate, 2008-14	-0.0025	47	0.006
	(0.0045)		

Table C1. Orthogonality of Judicial Foreclosure Requirement

	Judicial	N	R^2
GDP Variables			
Real GDP per capita annual growth, 2000	-0.0029	47	0.003
	(0.0077)		
Real GDP per capita annual growth, 2002	0.0042	47	0.011
	(0.0068)		
Real GDP per capita annual growth, 2004	-0.0008	47	0.001
	(0.0050)		
Real GDP per capita annual growth, 2006	-0.0021	47	0.002
	(0.0065)		
Real GDP per capita annual growth, 2008	0.0041	47	0.007
	(0.0074)		
Real GDP per capita annual growth, 2010	0.0050	47	0.013
	(0.0065)		
Real GDP per capita annual growth, 2012	0.0093	47	0.022
	(0.0104)		
Real GDP per capita annual growth, 2014	0.1039	47	0.089
	(0.0052)		
Real GDP per capita, 2000	1326	47	0.006
	(2614)		
Real GDP per capita, 2002	1850	47	0.012
	(2581)		
Real GDP per capita, 2004	1888	47	0.012
	(2635)		
Real GDP per capita, 2006	1367	47	0.005
	(2808)		
Real GDP per capita, 2008	1219	47	0.004
	(2870)		
Real GDP per capita, 2010	1916	47	0.009
	(2909)		
Real GDP per capita, 2012	2601	47	0.015
	(3122)		
Real GDP per capita, 2014	2748	47	0.018
	(3048)		
Real GDP per capita, 2016	1951	47	0.010
	(2922)		

Table C1. Orthogonality of Judicial Foreclosure Requirement (cont.)

	Judicial	N	R^2
Housing Market Variables			
House price growth, 2000	0.0046	47	0.003
	(0.1251)		
House price growth, 2002	0.0105	47	0.012
	(0.0142)		
House price growth, 2004	0.0101	47	0.006
	(0.0200)		
House price growth, 2006	-0.0070	47	0.005
	(0.1329)		
House price growth, 2008	0.0097	47	0.008
	(0.0163)		
House price growth, 2010	0.0089	47	0.019
	(0.0095)		
House price growth, 2012	-0.0277 **	47	0.161
	(0.0091)		
House price growth, 2014	-0.1372 *	47	0.087
	(0.0064)		
House price growth, 2016	-0.1044	47	0.034
	(0.0081)		
Manufactured housing shipping annual growth, 2000	0.0578	47	0.024
	(0.0521)		
Manufactured housing shipping annual growth, 2002	0.0566	47	0.046
	(0.0389)		
Manufactured housing shipping annual growth, 2004	0.0184	47	0.002
	(0.0576)		
Manufactured housing shipping annual growth, 2006	0.0397	47	0.010
	(0.0539)		
Manufactured housing shipping annual growth, 2008	0.0474	47	0.013
	(0.0622)		
Manufactured housing shipping annual growth, 2010	0.0893	47	0.016
	(0.1133)		
Manufactured housing shipping annual growth, 2012	-0.2223	47	0.012
	(0.3176)		
Manufactured housing shipping annual growth, 2014	0.2395	47	0.019
	(0.2948)		
Manufactured housing shipping annual growth, 2016	-0.0561	47	0.010
	(0.0813)		

Table C1. Orthogonality of Judicial Foreclosure Requirement (cont.)

	Judicial	N	R^2
Mortgage Market Variables			
Mortgage debt balance per capita (excl. HELOC), 2004	-3060	47	0.034
	(2332)		
Mortgage debt balance per capita (excl. HELOC), 2006	-3469	47	0.024
	(30594)		
Mortgage debt balance per capita (excl. HELOC), 2008	-4245	47	0.029
	(35645)		
Mortgage debt balance per capita (excl. HELOC), 2010	-3176	47	0.022
	(3115)		
Mortgage debt balance per capita (excl. HELOC), 2012	-2755	47	0.018
	(2993)		
Mortgage debt balance per capita (excl. HELOC), 2014	-2842	47	0.022
	(2756)		
Mortgage debt balance per capita (excl. HELOC), 2016	-2965	47	0.024
	(2731)		
Mortgage delinquency rate, Q42004	0.0020	47	0.031
	(0.0017)		
Mortgage delinquency rate, Q42006	0.0023	47	0.059
	(0.0014)		
Mortgage delinquency rate, Q42008	0.0044	47	0.013
	(0.0063)		
Mortgage delinquency rate, Q42010	0.0114	47	0.034
	(0.0098)		
Mortgage delinquency rate, Q42012	0.0186 *	47	0.140
	(0.0077)		
Mortgage delinquency rate, Q42014	0.0133 **	47	0.213
	(0.0043)		
Mortgage delinquency rate, Q42016	0.0074 ***	47	0.304
	(0.0018)		

Table C1. Orthogonality of Judicial Foreclosure Requirement (cont.)

This tables presents coefficients of the univariate regression for x_i variable on $Judicial$ dummy. Standard errors are heteroskedasticity robust. Coefficients marked with ***, **, * and $^{+}$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.
States subject to changes in the mortgage law not included in the regressions. Data sources: Demographics and income variables from Current Population Survey (CPS), GDP variables from Bureau of Economic Analysis (BEA), house price growth from Zillow - (Single Family Residence), manufactured housing shipping annual growth from Institute for Building Technology & Safety (IBTS), and mortgage market variables from State Level Household Debt Statistics 2003-2017, Federal Reserve Bank of New York.

	Non-recourse	N	R^2
Demographics and Income Variables			
Fraction with 65 years or more, 2008-14	-0.0104 +	47	0.085
	(0.0055)		
Fraction with less than high school, 2008-14	-0.0046	47	0.006
	(0.0109)		
Black fraction, 2008-14	-0.0613 *	47	0.083
	(0.0243)		
Hispanic fraction, 2008-14	0.0520	47	0.054
	(0.0399)		
Poverty fraction, 2008-14	-0.0047	47	0.005
	(0.0095)		
Fraction with personal income less than $25k, 2008-14	-0.0120	47	0.022
	(0.0110)		
Fraction with household income less than $25k, 2008-14	-0.0150	47	0.027
	(0.1132)		
Fraction with income less than $25k, 2008-14	-0.0082	47	0.008
	(0.0111)		
Unemployment rate, 2008-14	0.0019	47	0.003
	(0.0055)		
GDP Variables			
Real GDP per capita annual growth, 2000	0.0018	47	0.001
	(0.0091)		
Real GDP per capita annual growth, 2002	0.0031	47	0.005
	(0.0057)		
Real GDP per capita annual growth, 2004	0.0017	47	0.002
	(0.0058)		
Real GDP per capita annual growth, 2006	0.0087	47	0.029
	(0.0066)		
Real GDP per capita annual growth, 2008	-0.0006	47	0.000
	(0.0084)		
Real GDP per capita annual growth, 2010	-0.0007	47	0.000
	(0.0077)		
Real GDP per capita annual growth, 2012	0.0256 +	47	0.137
	(0.0144)		
Real GDP per capita annual growth, 2014	0.0042	47	0.012
	(0.0061)		

Table C2. Orthogonality of Non-recourse law

	Non-recourse	N	R^2
GDP Variables (cont.)			
Real GDP per capita, 2000	1266	47	0.005
	(2575)		
Real GDP per capita, 2002	1261	47	0.004
	(2641)		
Real GDP per capita, 2004	1468	47	0.006
	(2529)		
Real GDP per capita, 2006	2290	47	0.012
	(2746)		
Real GDP per capita, 2008	3064	47	0.020
	(2886)		
Real GDP per capita, 2010	3146	47	0.021
	(3071)		
Real GDP per capita, 2012	5392	47	0.054
	(3536)		
Real GDP per capita, 2014	5576	47	0.061
	(3372)		
Real GDP per capita, 2016	5206	47	0.056
	(3047)		
Housing Market Variables			
House price growth, 2000	-0.0074	47	0.005
	(0.0162)		
House price growth, 2002	-0.0071	47	0.004
	(0.0139)		
House price growth, 2004	0.0106	47	0.005
	(0.0213)		
House price growth, 2006	-0.0018	47	0.000
	(0.0139)		
House price growth, 2008	-0.0135	47	0.012
	(0.0214)		
House price growth, 2010	-0.0065	47	0.008
	(0.0121)		
House price growth, 2012	0.0272 *	47	0.127
	(0.0133)		
House price growth, 2014	0.0104	47	0.041
	(0.0071)		
House price growth, 2016	0.0088	47	0.020
	(0.0105)		

Table C2. Orthogonality of Non-recourse law (cont.)

	Non-recourse	N	R^2
Housing Market Variables (cont.)			
Manufactured housing shipping annual growth, 2000	-0.0027	47	0.000
	(0.0562)		
Manufactured housing shipping annual growth, 2002	0.0067	47	0.001
	(0.0355)		
Manufactured housing shipping annual growth, 2004	-0.0742	47	0.030
	(0.0553)		
Manufactured housing shipping annual growth, 2006	0.0347	47	0.007
	(0.0575)		
Manufactured housing shipping annual growth, 2008	-0.0274	47	0.004
	(0.0788)		
Manufactured housing shipping annual growth, 2010	0.2103	47	0.072
	(0.1582)		
Manufactured housing shipping annual growth, 2012	0.2080	47	0.008
	(0.3176)		
Manufactured housing shipping annual growth, 2014	-0.2128	47	0.012
	(0.1777)		
Manufactured housing shipping annual growth, 2016	0.0543	47	0.008
	(0.0863)		
Mortgage Market Variables			
Mortgage debt balance per capita (excl. HELOC), 2004	2851	47	0.024
	(2794)		
Mortgage debt balance per capita (excl. HELOC), 2006	4234	47	0.030
	(3886)		
Mortgage debt balance per capita (excl. HELOC), 2008	5199	47	0.036
	(4339)		
Mortgage debt balance per capita (excl. HELOC), 2010	4249	47	0.032
	(3583)		
Mortgage debt balance per capita (excl. HELOC), 2012	3573	47	0.025
	(3323)		
Mortgage debt balance per capita (excl. HELOC), 2014	3887	47	0.034
	(2958)		
Mortgage debt balance per capita (excl. HELOC), 2016	4149	47	0.039
	(2936)		

Table C2. Orthogonality of Non-recourse law (cont.)

	Non-recourse	N	R^2
Mortgage Market Variables (cont.)			
Mortgage debt balance delinquency rate, Q42004	-0.0034 *	47	0.077
	(0.0015)		
Mortgage debt balance delinquency rate, Q42006	-0.0029 *	47	0.078
	(0.0013)		
Mortgage debt balance delinquency rate, Q42008	-0.0014	47	0.001
	(0.0065)		
Mortgage debt balance delinquency rate, Q42010	-0.0016	47	0.001
	(0.0097)		
Mortgage debt balance delinquency rate, Q42012	-0.0077	47	0.020
	(0.0064)		
Mortgage debt balance delinquency rate, Q42014	-0.0084 *	47	0.069
	(0.0034)		
Mortgage debt balance delinquency rate, Q42016	-0.0053	47	0.130
	(0.0015)		

Table C2. Orthogonality of Non-recourse law (cont.)

This tables presents coefficients of the univariate regression for x_i variable on $NonRecourse$ dummy. Standard errors are heteroskedasticity robust. Coefficients marked with ***, **, * and $^+$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively. States subject to changes in the mortgage law not included in the regressions. Data sources: Demographics and income variables from Current Population Survey (CPS), GDP variables from Bureau of Economic Analysis (BEA), house price growth from Zillow - (Single Family Residence), manufactured housing shipping annual growth from Institute for Building Technology & Safety (IBTS), and mortgage market variables from State Level Household Debt Statistics 2003-2017, Federal Reserve Bank of New York.

Appendix D: Summary statistics by year

	Year 2001				Year 2002			
	mean	**sd**	**min**	**max**	**mean**	**sd**	**min**	**max**
Spread	63	40	-183	368	65	40	-304	496
Amount	149,984	61,564	50,000	578,000	157,388	67,663	50,000	650,000
Value	192,617	93,729	50,000	2,500,000	202,706	103,228	50,000	2,500,000
LTV	80.84	13.44	6	100	80.81	13.44	6	105
LTI	51.91	15.68	1.40	109.88	54.91	17.11	1.24	115.42
CreditScore	717	55	300	839	717	56	300	842
Income	3,218	2,386	480	212,338	3,266	3,284	474	200,256
DFirstTime	0.23	0.42	0	1	0.24	0.43	0	1
DSingleBorrower	0.37	0.48	0	1	0.40	0.49	0	1
Judicial	0.43	0.50	0	1	0.45	0.50	0	1
NonRecourse	0.39	0.49	0	1	0.39	0.49	0	1
Observations	445,097				395,960			

	Year 2003				Year 2004			
	mean	**sd**	**min**	**max**	**mean**	**sd**	**min**	**max**
Spread	61	42	-197	487	43	37	-192	457
Amount	166,905	73,920	50,000	749,000	169,673	76,714	50,000	642,000
Value	216,086	113,133	50,000	2,083,333	224,934	121,196	50,000	3,340,000
LTV	80.46	13.19	7	102	78.91	13.61	7	102
LTI	59.62	18.57	1.56	126.31	61.28	18.69	1.56	124.61
CreditScore	723	52	300	841	723	53	300	850
Income	3,149	2,560	434	230,512	3,069	2,219	429	264,417
DFirstTime	0.22	0.42	0	1	0.22	0.41	0	1
DSingleBorrower	0.39	0.49	0	1	0.40	0.49	0	1
Judicial	0.48	0.50	0	1	0.50	0.50	0	1
NonRecourse	0.36	0.48	0	1	0.34	0.47	0	1
Observations	346,957				313,324			

	Year 2005				Year 2006			
	mean	**sd**	**min**	**max**	**mean**	**sd**	**min**	**max**
Spread	81	39	-161	405	100	39	-160	381
Amount	178,777	83,738	50,000	800,000	186,532	92,168	50,000	802,000
Value	241,732	135,431	50,000	2,571,429	251,380	144,694	50,000	3,878,572
LTV	77.76	14.37	7	104	77.76	14.12	7	105
LTI	62.51	19.48	1.48	122.83	59.66	18.55	1.41	117.71
CreditScore	730	55	300	850	729	56	300	850
Income	3,183	2,292	431	219,423	3,467	2,490	456	182,283
DFirstTime	0.24	0.43	0	1	0.26	0.44	0	1
DSingleBorrower	0.42	0.49	0	1	0.42	0.49	0	1
Judicial	0.48	0.50	0	1	0.46	0.50	0	1
NonRecourse	0.34	0.47	0	1	0.35	0.48	0	1
Observations	352,502				301,679			

	Year 2007				Year 2008			
	mean	**sd**	**min**	**max**	**mean**	**sd**	**min**	**max**
Spread	93	38	-212	429	119	49	-161	389
Amount	189,786	96,030	50,000	802,000	211,741	109,237	50,000	802,000
Value	248,262	147,835	50,000	3,207,692	281,890	180,192	50,000	4,369,231
LTV	80.11	14.62	6	103	79.39	14.57	8	105
LTI	59.81	18.96	1.45	122.69	61.62	19.87	1.54	126.31
CreditScore	728	57	300	850	741	50	333	850
Income	3,535	2,700	482	254,811	3,834	3,056	425	298,465
DFirstTime	0.31	0.46	0	1	0.33	0.47	0	1
DSingleBorrower	0.45	0.50	0	1	0.49	0.50	0	1
Judicial	0.46	0.50	0	1	0.43	0.49	0	1
NonRecourse	0.35	0.48	0	1	0.40	0.49	0	1
Observations	269,851				212,152			

	Year 2009				Year 2010			
	mean	**sd**	**min**	**max**	**mean**	**sd**	**min**	**max**
Spread	136	46	-56	430	66	29	-73	304
Amount	218,730	123,981	50,000	1,403,000	218,706	124,904	50,000	1,129,000
Value	294,424	199,038	50,000	4,464,286	292,898	193,982	50,000	5,214,286
LTV	77.80	12.96	9	100	77.78	12.70	8	100
LTI	62.07	19.70	1.81	132.13	63.33	18.59	1.76	111.80
CreditScore	758	40	461	843	760	40	527	832
Income	3,908	3,012	413	220,680	3,777	2,843	461	249,622
DFirstTime	0.38	0.48	0	1	0.39	0.49	0	1
DSingleBorrower	0.48	0.50	0	1	0.47	0.50	0	1
Judicial	0.44	0.50	0	1	0.45	0.50	0	1
NonRecourse	0.40	0.49	0	1	0.40	0.49	0	1
Observations	208,588				162,476			

	Year 2011			
	mean	**sd**	**min**	**max**
Spread	64	34	-95	243
Amount	228,012	127,644	50,000	1,333,000
Value	298,860	193,853	50,000	3,037,500
LTV	79.47	12.69	10	100
LTI	64.87	18.27	1.81	127.77
CreditScore	760	39	562	835
Income	3,823	2,793	487	284,937
DFirstTime	0.37	0.48	0	1
DSingleBorrower	0.46	0.50	0	1
Judicial	0.44	0.50	0	1
NonRecourse	0.40	0.49	0	1
Observations	136,690			

Table D1. Summary statistics by year from 2001 to 2011

Appendix E: Distribution of residuals on the *Spread* regression

This appendix presents the residuals inspection for the estimation of Equation (1) with year and seller fixed effects. In the absence of normality tests valid for large samples, the

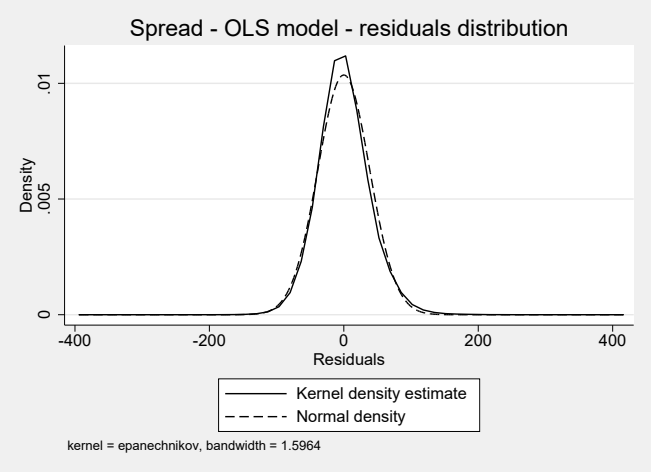

Figure E1: Residuals distribution on the $Spread$ regression

This figure presents the distribution of the residuals on the estimation of Equation (1) with year and seller fixed effects. The estimated residuals (Kernel density estimate) follow a distribution close to the assumption of normality distribution represented by the dash line.

Appendix F: Robustness checks

This appendix presents the main estimation results for the robustness tests. Table F1 shows the results for the alternative definition of $Spread2$. Table F2 shows the results for the estimation on the combined loan-to-value ratio. Table F3 shows the results when considering time dependency. Figure F1 shows the evolution of Case-Shiller U.S. National Home Price Index.

	Spread2	**Spread2**
LogIncome	-29.023**	-28.293**
	(9.104)	(8.531)
LogIncome^2	1.140*	1.112*
	(0.504)	(0.467)
LTI	-0.304***	-0.300***
	(0.017)	(0.014)
CreditScore	-0.137***	-0.136***
	(0.010)	(0.009)
DSingleBorrower	3.753***	3.642***
	(0.499)	(0.487)
DFirstTime	1.846*	2.600**
	(0.678)	(0.861)
Judicial	4.799***	4.414***
	(0.228)	(0.203)
NonRecourse	-0.133	0.141
	(0.181)	(0.244)
Level_lag1	-0.419***	-0.422***
	(0.012)	(0.013)
Slope_lag1	-0.793***	-0.798***
	(0.025)	(0.026)
Curvature_lag1	-1.046***	-1.055***
	(0.057)	(0.056)
Vol60d_lag1	0.610***	0.597***
	(0.077)	(0.076)
FE Year	yes	yes
FE Seller	no	yes
N	3,145,276	3,145,276
R^2	0.338	0.344

Table F1. Effects of law on Spread2

This tables presents coefficients of the OLS regression for $Spread2$ on law dummies—$Judicial$ and $NonRecourse$. Standard errors are clustered by seller. Coefficients marked with ***, **, * and $^+$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.

	CLTV	**CLTV**
LogIncome	53.162***	52.461***
	(4.053)	(4.023)
LogIncome^2	-3.284***	-3.227***
	(0.255)	(0.254)
LTI	0.053***	0.055***
	(0.004)	(0.005)
CreditScore	-0.050***	-0.050***
	(0.003)	(0.003)
DSingleBorrower	1.524***	1.567***
	(0.185)	(0.196)
DFirstTime	4.525***	4.706***
	(0.310)	(0.275)
Judicial	0.013	-0.060
	(0.208)	(0.188)
NonRecourse	-0.496**	-0.424***
	(0.147)	(0.109)
FE Year	yes	yes
FE Seller	no	yes
N	3,145,276	3,145,276
R^2	0.088	0.096

Table F2. Effects of law on CLTV

This tables presents coefficients of the OLS regression for $CLTV$ on law dummies—$Judicial$ and $NonRecourse$. Standard errors are clustered by seller. Coefficients marked with ***, **, * and $^+$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.

	Spread	**Spread**	**LTV**	**LTV**
LogIncome	-28.423**	-27.714**	49.087***	49.316***
	(9.584)	(9.038)	(2.716)	(2.706)
$LogIncome^2$	1.109*	1.090*	-3.066***	-3.062***
	(0.539)	(0.501)	(0.165)	(0.166)
LTI	-0.288***	-0.282***	0.041***	0.046***
	(0.015)	(0.014)	(0.006)	(0.006)
CreditScore	-0.133***	-0.132***	-0.055***	-0.055***
	(0.009)	(0.009)	(0.002)	(0.002)
DSingleBorrower	3.626***	3.551***	1.994***	2.014***
	(0.495)	(0.476)	(0.082)	(0.075)
DFirstTime	1.711*	2.489*	3.700***	4.066***
	(0.691)	(0.914)	(0.225)	(0.160)
2001xJudicial	6.461***	6.095***	0.036	-0.081
	(0.454)	(0.432)	(0.310)	(0.353)
2002xJudicial	3.284***	2.770***	0.189	-0.024
	(0.589)	(0.651)	(0.240)	(0.303)
2003xJudicial	5.037***	4.533***	0.433*	0.246
	(0.601)	(0.539)	(0.171)	(0.193)
2004xJudicial	5.949***	5.370***	0.498*	0.312
	(0.527)	(0.480)	(0.241)	(0.230)
2005xJudicial	4.971***	4.417***	0.566	0.389
	(0.699)	(0.607)	(0.325)	(0.247)
2006xJudicial	5.946***	5.564***	0.278	0.207
	(0.696)	(0.634)	(0.305)	(0.263)
2007xJudicial	5.697***	5.468***	0.195	0.129
	(0.603)	(0.613)	(0.366)	(0.318)
2008xJudicial	3.699***	3.599**	0.172	0.159
	(0.999)	(1.011)	(0.370)	(0.267)
2009xJudicial	3.798**	3.493**	0.359	0.211
	(1.289)	(1.095)	(0.326)	(0.215)
2010xJudicial	1.544*	1.071	0.536	0.441
	(0.660)	(0.808)	(0.384)	(0.345)
2011xJudicial	0.470	0.516	0.127	-0.013
	(0.659)	(0.931)	(0.389)	(0.393)

Table F3. Effects of law on Spread and LTV by Year

	Spread	Spread	LTV	LTV
2001xNonRecourse	0.130	0.621	-0.331 $^{+}$	-0.193
	(0.333)	(0.373)	(0.181)	(0.195)
2002xNonRecourse	-0.797	-0.397	-0.692***	-0.491*
	(0.571)	(0.602)	(0.184)	(0.213)
2003xNonRecourse	-0.556	-0.125	-0.553***	-0.377**
	(0.462)	(0.483)	(0.124)	(0.133)
2004xNonRecourse	-0.775*	-0.414	-0.465***	-0.326*
	(0.306)	(0.367)	(0.115)	(0.120)
2005xNonRecourse	-1.156*	-0.707	-0.608**	-0.452*
	(0.509)	(0.587)	(0.212)	(0.218)
2006xNonRecourse	0.022	0.460	-0.672**	-0.498*
	(0.400)	(0.426)	(0.227)	(0.187)
2007xNonRecourse	-0.690	-0.421	-0.784*	-0.594*
	(0.786)	(0.804)	(0.306)	(0.236)
2008xNonRecourse	0.556	0.245	-0.686*	-0.535^{+}
	(0.825)	(0.731)	(0.301)	(0.273)
2009xNonRecourse	2.130^{+}	2.008^{+}	-0.668*	-0.505
	(1.093)	(1.049)	(0.279)	(0.299)
2010xNonRecourse	1.358	1.279	-0.538^{+}	-0.383
	(0.931)	(0.891)	(0.288)	(0.300)
2011xNonRecourse	1.529	1.383	-0.645*	-0.554^{+}
	(1.073)	(1.067)	(0.297)	(0.288)
FE Year	yes	yes	yes	yes
FE Seller	no	yes	no	yes
N	3,145,276	3,145,276	3,145,276	3,145,276
R^2	0.327	0.333	0.096	0.106

Table F3. Effects of law on Spread and LTV by Year (cont.)

This tables presents coefficients of the OLS regression for $Spread$ and LTV on law dummies—$Judicial$ and $NonRecourse$—interacted with year dummies. Standard errors are clustered by seller. Coefficients marked with ***, **, * and $^{+}$ are statistically different from zero at the 0.1%, 1%, 5%, and 10% confidence level, respectively.

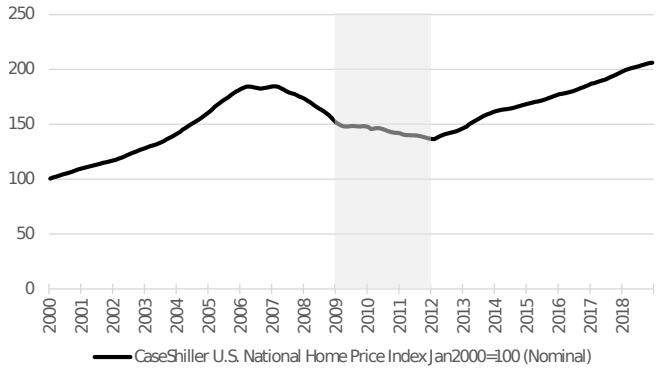

Figure F1: Case-Shiller U.S. National Home Price Index

Note: This figure shows the evolution of the Case-Shiller U.S. National Home Price Index data available on `https://fred.stlouisfed.org`. The shaded area emphasizes the years 2009, 2010 and 2011, for which the mortgage law have lower or no impact on the mortgage interest rate and on the LTV ratio. It coincides with a depressed housing market where lenders might attribute a low probability to the default scenario.

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